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THE PLACE OF SCIENTIFIC AND TECHNICAL SCHOOLS IN AMERICAN EDUCATION.[1]

BY FRANCIS A. WALKER, PRESIDENT OF THE INSTITUTE.

AMONG the vast changes in the spirit and life of our country, in the arts, the industries, the ideas, the aspirations, of the American people, which were brought about by, or which coincided with, the great struggle from 1861 to 1865, none is more remarkable than the rapid development of schools of applied science and technology. It is no part of my duty to name even the most important of these, or to attempt to divide among them the honor of what they have, as a whole, achieved. I shall confine myself to accounting, as far as I may, for the rapidity with which these schools have spread over the land, and to estimating their place in our educational system.

The nearest and easiest thing to say regarding the growth of scientific and technical schools, since the fortunate conclusion of the civil war, is that the industrial development of the country had reached the point where it had become necessary that the enterprises into which our labor and capital were to be put should be organized and directed with much more of skill and scientific knowledge than had been applied to our earlier efforts at manufactures and transportation; and so, in the fullness of time, scientific and technical schools came. In this view there is much of truth. The vaster enterprises of these later days, the ever increasing possibilities of modern commerce and industry, the intensifying severity of competition due to quickened com-

[1] Address at the Convocation of the University of the State of New York, Albany, July 9, 1891.

munication, fast mails, cheap freights and ocean cables, had indeed created an urgent want for greater technical skill and more highly trained intelligence. The old wasteful ways of dealing with materials, the rule-of-thumb methods of construction, the haphazard administration, characteristic of our earlier industrial efforts, could not have been continued without greatly retarding the national development and without irreparable loss in the result. But not, at the time spoken of, had this want become one of which our people were generally conscious; much less had it created a demand for such institutions which would of itself have sufficed to bring them into existence. The establishment of scientific and technical schools in the United States was to constitute a striking instance of the principle that, in some things, supply must create demand.

Economists and people generally are so much accustomed to think of the more usual condition in which demand creates supply, that they often forget — indeed, to many it never occurs — that there is another large class of cases, and these far the most important of all, in which the opposite rule obtains. In the lower ranges of life, in matters of clothing, food and shelter, and indeed in holding on to whatever advances civilization has once fairly and fully made, whether in material or in higher things, the conscious wants of humanity will in all ordinary cases suffice to secure the due supply, without any organized public or private effort other than that originating in personal interest. But in all things high and fine, and generally also in every advance which material civilization is to make, there must be a better intelligence than that of the market, which shall apprehend, not what the people want, but what they ought to want; there must be disinterested efforts on the part of the natural leaders of society, which shall secure, at whatever sacrifice, such a demonstration of the merits and advantages of the yet unknown thing, such a supply of the new good, as shall create the demand for it. It will not be until that want has been fairly and fully wrought into the public consciousness, that the supply may thereafter be left to take care of itself.

The American schools of technology illustrate in an eminent degree the law of human progress which I have stated. These schools did not come into existence in obedience to a demand for them. They were created through the foresight, the unselfish devotion, the strenuous endeavors of a few rich men, and of many very poor men, known as professors of mathematics, chemistry, physics, and geology. At the

time they came into existence there was a smaller demand for technically trained men than there is today, when for twenty-five years these schools have been pouring out their hundreds of graduates annually. That demand has been created by first furnishing the supply; by showing what young men properly educated and highly trained can do in organizing and directing the forces of American industry.

That these schools, in spite of the fact that they had everything to do at once and little to do it with; in spite of the fact that they had no traditions to govern them, and had, indeed, the whole philosophy of their subject to evoke, *a priori;* in spite of general public indifference and even of much contempt, have done their work exceedingly well, even from the first, is fairly implied in the foregoing statement. It is truly remarkable that, with so little to go by and so much to do, all at once, out of such scanty means, there should have been so little waste of effort, so little done injudiciously, so few steps taken that needed to be retraced.

Credit should also be given to the Congress of the United States for the act which was passed July 2, 1862, under the enlightened leadership of the Hon. Justin S. Morrill, of Vermont, making generous provision for the establishment, in the several States, of colleges of agriculture and the mechanic arts. Most of this provision was, it is true, devoted to the creation of agricultural schools, regarding which the scope of this address does not require me to speak, and, indeed, regarding which I should scarcely presume to express an opinion; yet the part which was assigned to the promotion of the mechanic arts proved to be a most valuable and timely reënforcement of the American system of technological schools.

But no one who thoroughly believes in the mission of schools of this class can be content merely to assert that the full time had come in the economic evolution of the nation when such schools were imperatively needed for the promotion of our industries, and that the institutions thus called into being have done this, their primary work, with triumphant success. We go far beyond this, and assert for these schools that they have come to form a most important part of the proper educational system of the country, and that they are today doing a work in the intellectual development of our people which is not surpassed, if indeed it be equaled, by that of the classical colleges. No statement less broad and strong than this would begin to do justice to the view we take of what these schools are now doing, and are in an

increasing measure to do, for the manhood and citizenship of the country. We believe that in the schools of applied science and technology as they are carried on today in the United States, involving the thorough and most scholarly study of principles directed immediately upon useful arts, and rising, in their higher grades, into original investigation and research, is to be found almost the perfection of education for young men. Too long have we submitted to be considered as furnishing something which is, indeed, more immediately and practically useful than a so-called liberal education, but which is, after all, less noble and fine. Too long have our schools of applied science and technology been popularly regarded as affording an inferior substitute for classical colleges to those who could not afford to go to college, then take a course in a medical or law school, and then wait for professional practice. Too long have the graduates of such schools been spoken of as though they had acquired the arts of livelihood at some sacrifice of mental development, intellectual culture, and grace of life. For me, if I did not believe that the graduates of the institution over which I have the honor to preside were as well educated men, in all which the term "educated man" implies, as the average graduate of the ordinary college, I would not consent to hold my position for another day. It is true that something of form and style may be sacrificed in the earnest, direct, and laborious endeavors of the student of science; but that all the essentials of intellect and character are one whit less fully or less happily achieved through such a course of study, let no man connected with such an institution for a moment concede!

That mind and manhood alike are served in a preëminent degree by the systematic study of chemistry, physics, and natural history, has passed beyond dispute. The haste with which the colleges themselves are throwing over many of their traditional subjects to make room for these comparatively new studies, shows how general has become the appreciation of the virtue of these, when combined with laboratory methods, as means of intellectual and moral training.

I have spoken of the characteristic studies of these schools as the best of all available means of both moral and intellectual training. I believe this claim to be none too broad.

1. The sincerity of purpose and the intellectual honesty which are bred in the laboratory of chemistry and physics, stand in strong contrast with the dangerous tendencies to plausibility, sophistry, casuistry, and self-delusion which so insidiously beset the pursuits of metaphys-

ics, dialectics, and rhetoric, according to the traditions of the schools. Much of the training given in college in my boyhood was, it is not too much to say, directed straight upon the arts which go to make the worse appear the better reason. It was always an added feather in the cap of the young disputant that he had won a debate in a cause in which he did not believe. Surely, to an audience in these more enlightened days it is not needful to say that this is perilous practice, if, indeed, it is not always and necessarily pernicious. Even where the element of purposed and boasted self-stultification was absent, there was a dangerous and a mischievous exaltation of the form above the substance of the student's work, which made it better to be brilliant than to be sound.

Contrast with this the moral and intellectual influence of the studies and exercises we are considering. The student of chemistry or physics would scarcely know how to defend a thesis which he did not himself believe. In that dangerous art he has had no practice. The only success he has hoped for has been to be right. The only failure he has had to fear was to be wrong. To be brilliant in error only heightened the failure, making it the more conspicuous and ludicrous. How wholesome to the mind and heart of the pupil is such a regimen!

2. Again, in addition to the graces of sincerity and intellectual honesty, which are the proper traits of physical and natural science; altogether aside, too, from any future technical uses to which the arts or the information acquired may be put, there is great virtue, as training for practical work in life, of whatever kind, in whatever sphere, to be found in the objective study of concrete things, which so largely makes up the curriculum of the schools we are considering.

3. Still another advantage which we claim for the characteristic studies of the new schools, is that, in a very large degree, they dispense with the system of examinations which has become the curse of modern education. The recent remarkable outburst in England, from educators of every name and class, against that system, justifies the strong terms I have used. It is admitted on all sides to be a problem of the greatest difficulty so to adjust the scheme of examinations that they shall not largely neutralize the good effects of sincere and straightforward study.

So far has cramming been carried in English universities, and even in our own colleges, that examinations have largely ceased to be a test

of the scholar's attainments, much more of his real proficiency in his studies. Students who have a marked facility in this sort of thing acquire, in time, the faculty of passing creditably examinations on matters of which they know almost absolutely nothing. By steadily cramming for a few days and nights, under artful coaches, who know the professors' weaknesses and fads, a young man exceptionally expert can "get up" a subject,[1] of which he would be troubled, the morning after examination, to give an intelligible account. A special organ — the examination organ — becomes developed, which is as specific as the water-sacks attached to the stomach of a camel, intended only to carry a certain amount of refreshment over a very dry place for a very short time. Indeed, the comparison fails to do justice to its subject. The examination organ is at once as specific and as external as the pouch of a kangaroo.

From this serious difficulty, schools of applied science and technology are, by the very nature of the case, largely freed. Indeed, the inapplicability of the scheme of examinations to the studies we are considering has even been made an argument against their introduction into universities. Prof. Parsons Cooke, in addressing a body of students at Harvard recently, said: "When advocating in our mother university of Cambridge, in Old England, the claims of scientific culture, I was pushed with an argument which had very great weight with the eminent English scholars present, and which, you will be surprised to learn, was regarded as fatal to the success of the natural science triposes then under debate. The argument was, that the experimental sciences could not be made the subjects of competitive examinations."

It is not true that chemistry and physics cannot be made the subject of examinations AFTER THEIR KIND; but it is true that, under competent teachers of these sciences, examinations have far less of the character of a cram, and far more of the character of a test of ability to do work. Moreover, in such a scheme of instruction, as a whole, examinations perform a much less important part, while the daily and

[1] I would not disparage the importance, as a professional accomplishment, of the ability to "get up" a subject in a very short time under high pressure. A lawyer has often occasion to do this very thing. But this is a professional accomplishment, and should be acquired as such. The period of professional study is not too late for the acquirement of this faculty. It can even be acquired later still, in the course of professional work. Such practice, however, in my judgment, forms no part of general education and training, and is only vicious and mischievous in the culture stage.

weekly exercises in the laboratory become continually of more and more account as a means of ascertaining the scholar's real progress. In this the schools of applied science and technology comply with the demands of modern thought in pedagogics. In no department of life more than in education is the Scripture precept of authority, "Let the dead bury their dead." The best examination which a student can pass is by showing his ability to *do the next thing.* If he can pass this examination successfully, the teacher need give little thought to what has gone before. And I venture, by the way, to suggest, with reference to the urgent inquiry now proceeding as to where the American youth loses two years of time in his preparation for college, whether a large part, if not the whole of that serious loss, is not sustained from the everlasting reviews and examinations through which the American teacher, alike in the primary, the grammar, and the high school, insists upon dragging the pupil three times a year or oftener, not only requiring him thus to be continually going over again ground once traversed, but, what is of more consequence, creating a sentiment throughout the school-room which inspires the scholars to be forever looking back, and not forward.

4. The last of the advantages attendant on scientific instruction which I shall enumerate, though the list might be still further extended, is found in a better relation between teacher and pupil. I would not willingly be guilty of exaggeration in this matter. With a really great and gifted teacher, the attitude of the scholar will always be that of respect and admiration, whether with or without affection and personal intimacy. But it cannot be denied that, in the traditional college, with the traditional subjects of instruction, the relation in question is likely to be less than a happy one. On the one side there is apt to be an undue assumption of knowledge, a tendency to dogmatism, and a too peremptory way of dealing with the pupil's doubts and difficulties. On the other side there is apt to be something of the tone of resistance, if not of resentment; a disposition to escape the teacher's scrutiny, if not to get around him with the petty tricks of the recitation room.

It would be foolish to assume that there is any virtue in the natural and physical sciences which will overcome the faults or deficiencies of mind and heart that are found in some teachers. There are men who will be priggish, pompous, and pretentious in doing anything. But there is a wonderful virtue in the studies we are considering for bringing teacher and scholar together in their work in a most simple, natu-

ral, and affectionate relation. He is the most successful teacher of science who puts himself in the attitude of discovering truth with his pupil, and of hunting with him for the object of their common search. Moreover, the very closeness of the contact in the laboratory of chemistry, physics, or mechanics, is such as to cause a continuous, insensible discharge of the electricity generated by the necessarily strict requirements of study and discipline, and thus to maintain the friendly relations of teacher and pupil, unbroken by those storms which sometimes gather and burst in colleges where the teacher sits, buttoned-up, on a platform, behind his desk, and lectures to his pupils from the chair of authority.

But it may be said: Considering all that may be claimed for the purely educational advantages of the scientific studies which run through the curriculum of the technological schools, why may not all these advantages be equally obtained by the student of the traditional college, and even to better effect, since there he may secure the pure gold of truth freed from the alloy of baser metal — by which term the critic would designate the useful, practical applications of science. It is here that it behooves us to take issue, most directly and aggressively, with those who assert for the old-fashioned colleges an educational virtue superior to that of the schools we represent. It is of the very essence of our case that the directness and immediateness of application to which the studies of our pupils are subject, under their very eyes and at their very hands, constitutes a tremendous educational force, securing a closeness and continuity of attention on the part of the pupil, an earnestness of effort, a zeal and enthusiasm of work, which it is utterly beyond the power of the teacher of classics or philosophy to arouse, except in the case of gifted students. If proof of this upon a large scale be needed, it is enough to refer to the well-known fact that law schools and medical schools invariably command the energies of their pupils in a far higher degree than do the colleges; and that hosts of young men who have idled and dawdled away the four years nominally devoted to classics and philosophy throw themselves with splendid enthusiasm into their professional studies when once they, for the first time, see upon what ends their efforts are directed, and how their energy and application are to promote their happiness and usefulness in life.

Even in the case of those young men who need no such incentive to secure their faithful attention and earnest endeavor, we yet hold

that schools of applied science and technology possess a distinct advantage, in that their students learn the truths of science in a somewhat different way, and, in the result, know them somewhat better than do those who study these truths, no matter how diligently, without immediate, direct, and constant reference to their applications. Without referring further at this point to the limitations and defects inherent in all academic systems of recitation and examination, I believe it to be true that the man who, in studying mathematics, for example, has only to look forward to a recitation tomorrow and an examination two weeks or two months hence, applies himself to the subject necessarily in a different spirit and also necessarily with an inferior result, in contrast with the man who, continually as he acquires his mathematics, puts it to use day by day in the laboratory of physics, mechanics, hydraulics, or steam engineering.

For these reasons we must decline to accept the characterization of the technical applications of science as the alloy which debases the pure gold of truth. We look upon them, the rather, as the tough, elastic bow which sends the keen shaft to its mark. And, be it remembered, zeal and enthusiasm of work are not to be valued merely because, or merely as, they secure directness of attention, continuity of application and sustained endeavor. In themselves, of themselves, they are in a high sense an educational force, telling immediately and telling powerfully upon intellect and character, contributing importantly to build up mental and moral substance firmly and healthily.

There is one school in the United States mainly devoted to the application of scientific principles to a professional art, which is so well known to all our people, and whose work in the development of mind and manhood has been so severely tested in the sight of the country and of the whole world, that I cannot forbear to allude to it here. I mean the Military Academy at West Point. There is no reason to believe that, for the thirty years preceding the civil war, the young men who went to that school were in any degree superior to those who entered Yale or Harvard. Indeed, there was at that time, at least throughout the North, a certain disinclination on the part of the more generous and ambitious of our youth to adopt the career of arms. Yet when the war broke out, what a wealth of intellect and character was displayed by the graduates of that one small school during the terrific trial to which they were instantly and without preparation subjected! Think how many men from that single academy, which had fewer

living graduates than either Amherst or Williams, led army corps and armies with distinction, on the one side or the other, in what was perhaps the greatest war of modern history! I said "of intellect and character," for it is character, even more than intellect, which enables the commander to bear the tremendous cares, responsibilities, and burdens of his office. What was it which, out of those few small classes of raw lads, developed a Grant, a Lee, a Sherman, a Meade, a Jackson, a Thomas, the two Johnstons, a Hancock, a Reno, a Reynolds, and a Sheridan, not to mention scores of others who "waxed valiant in fight" and commanded divisions and corps with a skill and address which have excited the admiration of the professional soldiers of Europe? Doubtless in some part it was the romance and the highly stimulating influences of the military career. Doubtless in part also it was the special inspiration of the tremendous occasion, fraught as that was with the destinies of a continent. But I believe it, in still greater part, to have been the perfectly natural effect of the application of perhaps not extraordinary powers to the thorough, patient, unremitting study of scientific principles, directed straight upon a worthy profession, under the tuition and guidance of renowned masters of that art, and under the constant influence of professional ideas, professional sentiments, and great professional examples.

A great deal more might be said in comparison of the influence of scientific teaching as carried on in the schools of applied science and technology, with the influence of the traditional, or of the more modern, modified, curriculum of the classical colleges; but perhaps enough has been said to justify the assertion that the former class of institutions are just as truly educational as the latter. Here I am content to rest my case. This conceded, let the youth of the land seek the one or the other kind of school, according to their individual tastes, predilections, and plans for life. I am far from being so bigoted as to suggest that there is not room enough in the educational system of the future for all the institutions of the elder type which have achieved for themselves a name in letters and philosophy; which have, with pains inexpressible, wrought out their own problems and created their own constituencies; and each of which has a host of eager, devoted alumni, ever turning gratefully to the halls in which they were nurtured, and delighting to give to the old college the fruit of their labors and the fruit of their loins. But I confidently look to see a largely disproportionate number of the new institutions which shall from time

to time come into being, built essentially upon the plan which has achieved such prodigious successes during the quarter-century now closing. Doubtless the present general scheme of the schools of technology will itself undergo considerable modification, alike from the results of added experience, from larger means, and from the infusion of a wiser and more generous spirit. Doubtless more of economic, historical, and philosophical studies will be introduced to supplement, by their liberalizing tendencies, the work of the sciences in making their pupils exact and strong. Possibly some ultimate form for institutions of the higher learning may yet be developed, which shall embody much of both the modern school of technology and of the old-fashioned college, with, perhaps, something taken from neither, but originating in the larger, fuller, riper life of a happier and richer future.

SOME CONSIDERATIONS REGARDING HELMHOLTZ'S THEORY OF CONSONANCE.[1]

By CHARLES R. CROSS and HARRY M. GOODWIN.

THE present paper contains the results of a number of experiments relating to certain aspects of the theory of consonance and dissonance put forth by Helmholtz in the *Tonempfindungen*. In that work the author gives as the number of beats producing the maximum of harshness from thirty to forty per second, reaching this conclusion from a consideration of the amount of dissonance of various different chords in different octaves.

Mayer (*American Journal of Science*, **108**, 241; **109**, 267) studied the relation of the number of beats causing the greatest harshness to the absolute pitch, and showed that there is a marked rise in this number as the pitch becomes higher. The beats studied by Mayer, however, as he himself pointed out, are in several respects different from those which occur in the case of mistuned unisons. They were caused by rotating a disc, which was provided with a circle of holes near the circumference, between a vibrating tuning-fork and a resonator, the number of beats per second being of course greater as the speed of the disc was greater. Hence the sound-wave reaching the ear was not at all a compound wave due to the superposition on one another of two simple sound-waves slightly differing in length, as in ordinary beats, but merely a series of simple waves with gaps, as it were; while the passage from maximum to minimum intensity in the sound reaching the ear was very different in its character from that actually occurring with ordinary beats. In referring to such beats as those studied by Mayer, we shall for the sake of brevity designate them as "interruptional beats," and the ordinary beats we shall call "interference beats."

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[1] Read at a meeting of the American Academy of Arts and Sciences, June 10, 1891.

We have thought it desirable to ascertain what results would be reached in different registers with interference beats, such as are present in actual dissonances. It was of course necessary to employ simple tones. Our method was a direct one, viz., to produce two such tones simultaneously, and to vary the pitch of one of these by known amounts, estimating by the ear when the sound became the harshest.

As a source of sound we employed the cylindrical resonators of Koenig, which were sounded by gently blowing across the opening a stream of air from the brass, slit-shaped "universal embouchure" made by the same maker. We found these tones to be very pure and loud, while their pitch could be varied with great readiness without altering the intensity of the sound emitted. The set of resonators employed ranged from Sol_1 to Mi_5, the ranges of successive resonators overlapping slightly. The two embouchures were connected to a *Y* by rubber tubes, and placed on adjustable supports in suitable relation to the apertures of the two resonators to be sounded. The strength of the blast was regulated by a stopcock, and the bore of each rubber tube could be closed more or less by a clamp, so that the force of the jet of air at each resonator could be adjusted with all needed delicacy. Thus the intensity of the notes given out by the two resonators could be adjusted so that they were exactly equal in loudness. This was done by causing the tones to beat slowly, and so adjusting the apparatus that there was apparently an absolute silence when the sound fell to its minimum intensity. No difficulty was found from any whirring of the sheet of air as it struck the edges of the aperture, since by proper adjustment this could be made quite inappreciable. Having once made the necessary adjustments the pitch of either note could be varied by drawing out or pushing in the lower half of the resonator. The actual pitch of the note given by the resonator was in all cases determined by comparison with a tuning-fork of known rate. As the pitch of the resonator when blown is to a certain extent dependent on the position of the embouchure and the strength of the blast, it would not be safe to assume that the actual note given by the resonator was that marked upon it. Neither resonator when sounded sensibly affected the pitch of the note given by the other.

The determination of the point at which the harshness of the dissonance produced by the tones of the two resonators reaches a maximum is of course a very difficult problem to be solved exactly. It is,

moreover, a question still to be investigated, whether precisely the same results would be reached by different observers possessed of equally good perception. The following figures given in Table I were ascertained from a long series of observations by one of the writers (Mr. Goodwin). They are liable to an error of a single beat in either direction, possibly somewhat more in one or two cases. They furnish, however, the first series of direct measurements of this kind which we know.

TABLE I.

Ut_2	Ut_3	Sol_3	Ut_4	Sol_4	Ut_5	Mi_5
19	26	29	32	36	41	45

It will be noticed that the result for Ut_4, 32 beats per second, agrees very closely with the number 33, which Helmholtz gives for maximum dissonance in the middle registers. He found that of the ordinary musical intervals, b' c'', as he designates it, was the most dissonant, and as this gave 33 beats per second, he considered this number as giving the harshest effect for tones of corresponding pitch.

The notes furnished by the resonators are also well adapted for the direct study of the limit at which the beats cease to be audible when their number is increased. As the interval between the beating notes is increased, the beats heard become fainter and fainter until they disappear. It is all but impossible to fix upon the precise point of disappearance, since the change in loudness of the beats is so gradual. But, as will presently appear, our results clearly show that the maximum number of beats perceptible, like the number giving the maximum dissonance, varies greatly with the pitch of the notes sounded.

The pitch of one of the resonators was varied so as to run upward and downward through the vanishing point of the beats. This procedure was extremely tiresome, and the values obtained were found to be considerably influenced by fatigue of the ear. The following table (Table II) gives the results obtained when the ear was in good condition. The first column gives the rate of vibration of the lower note, the next four the rate of the upper note as found in corresponding series of observations; the seventh column gives the number of beats, and the last the interval between the two notes.

TABLE II.

Lower Note.		Upper Note.				Mean.	Beats.	Interval.
		1.	2.	3.	4.			
Ut_2	128	175	170	177	173	174	46	Fourth +
Ut_3	256	328	321	326	329	326	70	Major third +
Sol_3	384	467	458	469	460	464	80	Major third — 16 vibrations.
Ut_4	512	620	616	618	611	616	104	Minor third (nearly).
Sol_4	768	882	890	886	890	887	119	Tone + $\frac{1}{4}$ tone (nearly).
Ut_5	1024	1160	1167	1160	1163	1163	139	Tone + 11 vibrations.
Mi_5	1280	1420	1424	1421	1418	1420	140	Tone — $\frac{1}{3}$ tone (nearly).

These figures abundantly confirm for interference beats the results already reached by Mayer for interruptional beats, viz., that the ear is capable of appreciating a greater number of beats as the pitch of the beating notes rises. They also show very beautifully the increasing consonance of small intervals as the pitch rises, noted by Helmholtz.

The numerical results reached by us differ, as a whole, considerably from those given by Mayer for interruptional beats in either of his papers, though the divergence is less than that between the two series representing his own observations and those of Mrs. Seiler. In some cases, however, the agreement is very close. Thus for Ut_2 we find the last trace of beats to occur when these are 46 in number. The number as observed by Mayer is 26, and as observed by Mrs. Seiler, 45. For Ut_3 our figure is 70, and Mrs. Seiler's is 70 also. For the other notes the divergence is wide, except for Ut_5, for which our results agree quite well with those obtained by Mayer himself. These differences may be due to the different way in which the variations of intensity in the beats progress, to the presence of pitch variations in interference beats, to the fact that in our method the musical interval of the beating notes increases as the beats increase in number, to mere differences in the estimation of the observers, or to some other less evident causes.

We find that the ratios between the number of beats producing the greatest harshness at different pitches, and the maximum number of beats discernible at those pitches, are as follows (Table III). The numbers given are obtained by dividing the former number by the latter.

TABLE III.

Ut_2	Ut_3	Sol_3	Ut_4	Sol_4	Ut_5	Mi_5
0.41	0.37	0.36	0.31	0.30	0.30	0.32

These figures, as a whole, agree very well with the ratio (four tenths) given by Mayer, although the absolute numbers whose ratios are taken are quite different from his. Our results also seem to indicate a clearly marked fall in this ratio as the pitch rises.

A few experiments were made using Professor Mayer's method, with some slight modifications, in order to observe the judgment of the same ear upon beats of different kinds. The tuning-fork employed was kept in vibration continuously by electricity, and the rotating disc was driven by an electro-motor, which secured great constancy of speed. We were seriously troubled by the sound made by the rotating disc, which produced a sort of siren effect as its openings passed before the aperture of the resonator. For Ut_3 the maximum number of intermittences perceived was 50 per second. For Ut_4 the number was about 106. These figures are liable to an error of perhaps two beats, or even a little more for the higher pitch. Mayer's figures are 47, 78, respectively, for his own observations on these notes, and 70, 130, for those of Mrs. Seiler. It is clear that there are very great differences in the sensitiveness of different ears for the perception of rapid beats.

In another series of experiments interruptions were produced by a break-wheel placed in a telephonic circuit with a magneto-telephone at each end. The sound of a tuning-fork was transmitted through the line, and any desired number of interruptions could be produced by varying the rate of rotation of the wheel. Considerable annoyance was experienced at times from sounds which apparently arose from microphonic action at the break-wheel. These can probably be avoided in future experiments by causing the wheel to divert the current from the receiver periodically, instead of interrupting it. The limits at which the beats disappeared, with the notes experimented upon, were as follows: Ut_4 48, Mi_4 56, Sol_4 69. These figures are the means of a number of observations, and are apparently true within less than one beat per second. Comparing this kind of interruptional beat with those given by Mayer's method, the maximum number perceptible seems to be smaller for the former than for the latter. This is probably due in part to the greater loudness of the sounds observed in the

latter case, but there is another marked difference which must exercise an important influence upon the phenomenon under consideration. With the beats produced by the break-wheel the sound begins and ends almost instantaneously, so that sharply marked intervals of sound and silence of equal duration succeed each other. One would naturally expect the telephonic beats to be the more distinct of the two, which is not the case. The subject is one which requires further study as to the influence of the relative duration of sound and silence in the telephonic method, and of the relative size of the apertures in Mayer's method.

In the matters which we have thus far discussed in the present paper, our results are fully in accordance with the theory of consonance proposed by Helmholtz. In some other particulars, however, we are led to conclude that this theory is incomplete, at least.

Some years ago in a paper read at the Philadelphia meeting of the American Association for the Advancement of Science, and of which an abstract was published in the Proceedings of that Society for 1884 (page 113), one of us called attention to the bearing of certain phenomena of binaural audition upon Helmholtz's theory, as follows:

"In connection with his study on the effects of beats in causing dissonance, Helmholtz considers the condition of the vibrating portions of the inner ear as to resonance and damping. In his remarks on the subject he assumes that no notes are capable of beating with each other unless they both affect the same vibrating element of the inner ear. This view of the matter gives a purely mechanical action in the ear as the explanation of the physiological phenomenon of beats.

"In addition to the experiments of Koenig, in which beats were obtained between notes of very great intervals, there are certain phenomena of binaural audition which appear to prove that this view is incorrect, and that rather than, or at least in addition to, this mechanical interference of vibrations in the ear there is a more obscure operation within the sensorium itself. I refer to the fact that beats may occur when the exciting sounds operate upon different ears, and also apparently between the after-sensation and the succeeding sound of mistuned unisons. Whether this last phenomenon can be observed when the after-sensation results from an impression made upon the opposite ear to that which is impressed by the succeeding sound, I do not know; but in view of the fact that two simultaneous sounds acting upon different ears may beat, I do not see why this result should not be possible.

"Now if beats arise between the after-sensation and a following sound, Helmholtz's view can be true only upon the supposition that the vibrating parts of the inner ear continue in motion as long as the after-sensation persists, and that there is no residual sensation capable of giving beats other than that which persists only as long as the actual vibration in the ear itself continues. Such a supposition seems quite improbable. Moreover, the phenomenon of beats in the case of sounds acting simultaneously upon different ears cannot be explained even upon this supposition. There must be some kind of *vibration*, using this term in the most general sense, or some kind of alternation of phase or state within the sensorium itself.

"There is no question as to the fact of the production of beats under the circumstances last mentioned. I have not only verified the fact by experiments conducted in the usual manner, but have also studied the beats produced when a tuning-fork is held close to one ear and the sound of a second fork, not quite in unison with it, is transmitted by telephone to the other, taking suitable precautions that the ear against which the receiving telephone was held was not affected by the sound of the neighboring fork. Beats were readily obtained, which grew weaker as the fork was moved away from the open ear and approached to the ear which was closed by the receiver. The same result occurred when, instead of mistuned unisons, harmonic forks were used, which gave beats with the fundamental."

In the experiments just cited, the fact that the fork gave feebler beats as it was moved towards the ear to which the telephone was applied seemed clearly to indicate that the beats were not likely to be due to sounds transmitted through the head from one ear to the other, or to the passage of the vibrations through the skull from the outer air. These experiments did not differ in other respects from those of previous observers. The beating of the sound persisting in one ear after excitation had ceased with a second sound falling upon the other ear was, in fact, observed by S. P. Thompson in 1881.

The existence of such binaural beats as are under consideration was noted as early as 1874 by Mach, who assumed that the sound was conducted from one ear to the other through the bones of the head. S. P. Thompson has studied the subject very carefully (*Philosophical Magazine*, **4**, 274; **6**, 383; **12**, 351; **13**, 406), and while he is evidently strongly inclined to believe that the phenomena are due to an interference of sensations not produced by the same ear, nevertheless

he does not appear to consider this as a fact finally settled beyond all question, although to us his results seem to leave small room for doubt on the matter.

The important bearing of the existence of binaural beats upon some parts of Helmholtz's theory of audition does not seem to have been very fully recognized. This may be because of some question as to whether each sound did not after all really act upon both ears, even when apparently applied to one alone. From the nature of the case it is very difficult to prove finally that there is no possibility of such conduction of sound from one ear to the other, but we have sought to study the phenomena of binaural beats under circumstances which render such conduction improbable in the highest degree. With this end in view we have repeated most of the experiments of Thompson, and made such additional tests as suggested themselves to us.

The sounds of two tuning-forks placed in a distant room were conveyed to the ears telephonically over two separate circuits. These forks were struck by hammers actuated by electro-magnets placed in circuits separate from those containing the telephones, and governed by keys operated by the foot of the observer at the receiving end of the line. This method was found to be preferable to running the forks continuously by electro-magnetic means, as the currents used in the latter method were liable to act directly upon the transmitting telephone and cause disturbing noises. Very powerful magneto-telephones were used as transmitters and receivers. Mi_4 forks were used throughout the experiments, unless otherwise stated, on account of the remarkable intensity with which their tones were transmitted, this being approximately the natural tone of the diaphragm. The singular phenomena of the localization in the back of the head of the sensation caused by two notes in opposite phases when heard by separate ears, and the apparent wandering of the sensation from one ear to the other in the case of binaural beats referred to by Thompson, were always clearly recognized.

The method of experimentation precludes any fusion of the two sounds, except within the head; but to decide whether the beats arise from an interference of the sensations or from a mechanical interference in Corti's organ, we must investigate further. We tried to throw some light on this point by the following experiments. It is well known that the vibrations of a fork, even when of too small amplitude to be heard when held opposite the ear itself, can be made

audible by pressing the stem of the fork against the bone of the skull, or, still better, against the teeth, the latter transmitting vibrations to the ears better than any other part of the head. A still more effectual means of making audible very small vibrations is to close the ear with a bit of beeswax and press the stem of the fork lightly against the wax. In this case the vibrations are transmitted to the membrana tympani by the small amount of air inclosed within the meatus, as is clear from the fact that the sound of the fork is heard on touching the wax long after it ceases to be audible on touching its stem to the pinna of the ear. Hence in this case there is no conduction to the middle or inner ear through the bones of the head. Whatever sound reaches the ear follows the ordinary path through the meatus to the membrana tympani. Now we found that the vibrations of a fork could be heard longer when touched to the wax in the ear than when held against the teeth. We therefore took two small tonometer forks making four beats per second, struck them very gently and held their stems against the teeth; loud beats were heard in the ears just as they are ordinarily heard when powerful forks are sounded in the air. The forks were then held in this position until the beats had entirely ceased to be audible, when they were removed, and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats. In trying this experiment care was taken that no vibration should be imparted to the fork from a gentle blow given on touching its stem to the wax, and it was found that the liability of such an occurrence could be greatly diminished by covering the end of the stem with a layer of wax.

To make sure that these beats were not the result of imagination we took forks giving four, eight, twelve, and sixteen beats per second at random, struck them lightly as before, but in this case held only one of them against the teeth, so as not to know the number of beats which ought to be produced. When this fork had ceased to be heard, we touched both forks to the wax in the ears, as before, and in every case the correct number of beats was at once heard. The experiment was varied slightly as follows: One ear only was closed with wax; the other was immersed in a large basin of water. The experiment was then repeated as above, with the difference that one fork, instead of being touched to the ear, was touched to the marble basin, its vibra-

tions being transmitted to the inclosed ear through the water. The same results were obtained as before. These experiments were all carried on at night when there was not the slightest disturbing vibration in the air. Together with another experiment described below, they lead us to conclude that aërial vibrations acting upon the ear are not transmitted through the skull or bony parts of the head from one ear to the other. The experiments cited certainly seem to bear out this view, at least for tones of feeble intensity. Furthermore, the fact that differential tones have not been produced binaurally leads us to believe that it is true generally, even for very intense sounds, that binaural beats do not result from sound conduction within the head.

In his experiments Thompson was never able to obtain differential tones unless the sounds were allowed to mingle before falling upon the ears. Our experiments fully bear out this result, which we have verified in the following ways: (1) By listening to Ut_4 and Mi_4 forks through the telephones. These notes were transmitted with more than sufficient intensity to give rise to a differential when heard in the ordinary manner; but with a telephone placed at each ear not the faintest trace of such a tone could be heard. We also reached equally negative results on listening to Mi_4 with a telephone, and holding an Ut_4, Sol_4, or Sol_3 fork close to the other ear, or on leading their vibrations to the ear through a long rubber tube. (2) By Thompson's method of carrying the tones to the ears by means of rubber tubes (in our experiments about thirty feet long), the forks being struck, one in another room and the other outside of the window, to prevent their tones from mixing before reaching the ears. No differential tone could be heard. We also tried to use Koenig's forks for illustrating beat-notes, but their very rapid vibrations were so deadened by the tubing that they could only be heard very faintly. (3) By the same process as above, with the exception that the ends of the rubber tube were connected with cylindrical resonators which were blown as already described. This gave almost deafening tones in each ear, but not the slightest trace of a differential. But in this case, as also in (2), if the two tubes were connected to two branches of a *Y* tube, and a third rubber tube leading to the ear was connected to the third branch, the differential tone came out at once. (4) The most striking proof was the following: The ears being closed with wax, a brass rod about five feet long was held lightly against the wax in each. When the stems of forks struck by two assistants were pressed against the farther end of the

rods, very loud tones were heard in the ears, unaccompanied by any differential tone. If, however, one of the rods was removed from the ear and pressed tightly against the head, or better, against the teeth, a loud differential tone was heard at once in the ear against which the rod was placed. If both rods were held against the teeth or head the differential tone was heard in both ears.

This apparent impossibility of producing differential tones when the primary tones are caused to actuate separate ears would seem conclusive of the point under discussion, except for the fact that it might reasonably be argued that if sound conduction through the skull occurred from one ear to the other, the vibrations thus conducted might act directly on the inner ear, and not indirectly through the membrana tympani and chain of bones, in which case no differential tones should arise, although beats could still take place in the manner assumed by Helmholtz. But our experiment with the brass rods, described above, shows that such tones are in fact readily produced when conduction to the ear takes place through the bones of the head. And if there were any material conduction of this kind, it certainly seems as if the very loud sounds used by us would have given evidence of it at least by the occasional production of differential tones when the sounds actuated separate ears.

The production of beats without the simultaneous action of the beating sounds upon the same ear seems, therefore, to be clearly shown from these facts: (1) Beats are produced by sounds falling upon the ears separately even when the greatest precautions are taken to prevent both from affecting the same ear. (2) Under no circumstances does it seem to be possible to produce differential tones by binaural combination, even when the separate tones are extremely loud. (3) In all cases in which the beating sounds have apparently acted separately upon the auditory apparatus of each ear, the singular phenomenon of encephalic localization of the beats has appeared, while this phenomenon has never been found present either when the two sounds acted simultaneously upon the same ear through the auditory canal, or when one of them was manifestly transmitted to the ear through the bones of the skull.

We are therefore led to the conclusion that the mechanism in which the interference causing beats takes place, and through the operations of which we consequently recognize the character of consonant and dissonant intervals, lies in part, at least, beyond the audi-

tory apparatus of the inner ear. It seems to us clear that notes may beat when they do not affect the same vibrating elements of the inner ear, and that we must look to some more profound changes within the sensorium itself for a complete explanation of the phenomena under consideration.

It may be possible that there are two distinct causes of beating; one, that suggested by Helmholtz, which wholly or principally obtains when the two notes act simultaneously upon the same ear, and the other due to some different and more obscure action which obtains in the case of binaural beats. In this case quite different laws as to consonance might hold. The exceeding harshness of dissonances in binaural audition lends some support to this view.

ROGERS LABORATORY OF PHYSICS,
May, 1891.

FLOOD DISCHARGE FROM SMALL WATERSHEDS.

By DWIGHT PORTER, Associate Professor.

As cities grow, as manufacturing develops, as cultivation of the soil extends, a demand constantly arises for the impounding of water in reservoirs, to furnish a supply for domestic use, or for power, or for the crops in arid regions, and the country rapidly becomes dotted with the artificial lakes and ponds thus created. Commonly they are at the head waters of the small streams, where the latter are most readily controlled. Along the valleys below may be thriving villages and populous towns, liable to be inundated, and perhaps swept away at any moment, except as the vast energy of the waters stored above is restrained by an earthen or masonry dam.

Above the South Fork dam, by the failure of which, on May 31, 1889, more than two thousand lives were lost at Johnstown and its vicinity, and property to the value of millions of dollars was destroyed, were stored about 20,000,000 cubic yards of water. For years the waters of the reservoir were held back by a well-built embankment, until at length their opportunity came, and deliberately, almost leisurely, yet relentlessly, they cut their way through. In their report upon the failure of this dam,[1] Messrs. Francis, Worthen, Becker, and Fteley, a committee of the American Society of Civil Engineers, make this significant remark: "There are today in existence many such dams which are not better, nor even as well, provided with wasting-channels than was the Conemaugh dam, and which would be destroyed if placed under similar conditions." The conditions there present were, briefly stated, a substantially constructed earthen dam, a full reservoir, an extraordinary rainfall, an insufficient waste-way.

It is hardly conceivable that an earthen reservoir embankment would fail by sliding or by overturning as a whole, although such action is entirely possible with a timber or a masonry dam. Rather is the earthen structure likely to be overcome through the insidious

[1] Transactions of American Society of Civil Engineers, 24, 457.

action of a small current of water, finding for itself a passage through or over the embankment, and then with increasing speed enlarging this channel by erosion, until the trickling stream becomes a resistless torrent.

The percolation of water through an embankment is guarded against by careful selection and compacting of the material of the structure; and further, in general, by building at the center an impervious wall. The overtopping of the dam by water is designed to be prevented by providing a sufficient waste-way to carry off surplus storm water without allowing the reservoir surface to rise dangerously near the crest of the dam. The waste-way may be either a weir, over whose edge the surplus water shall escape to a suitable channel below the dam, or it may be a gate or set of gates, discharging into a pipe, tunnel, or other outlet. The important question arises, How large a discharging capacity shall the waste-way receive? Since the quantity passing a given weir or orifice under a given head can be computed with sufficient accuracy, the problem reduces to finding the volume of water for which it is necessary to provide an outlet. In other words, assuming the reservoir full to the safety line, what will be the probable maximum rate of discharge into it from the tributary watershed?

Clearly, the rate of greatest discharge must be largely dependent upon the amount of precipitation in severe storms. The amount and distribution of rainfall are matters either of record within, or closely adjacent to, the watershed under consideration, or of inference from the records of other and more or less distant localities. From the accompanying memoranda, which are intended to show the precipitation in a few notable storms, probably among the severest on record in the respective localities, it will be noticed that on the Atlantic coast rainfalls of from 6 to 10 inches in from twenty to sixty hours in single storms have repeatedly been observed; and that in the South and on the Pacific coast, even those figures have been far exceeded:

Storm of October 3–4, 1869. General over eastern part of United States. Rainfall heaviest in Connecticut. Maximum recorded rainfall during storm 12.35 inches, at Canton, Connecticut. At various stations 8 or 10 inches of rain fell in from thirty to forty hours, and upon an estimated area of more than 24,000 square miles the total precipitation was 6 inches or more, occurring in from thirty to fifty or sixty hours.

JAMES B. FRANCIS,
Trans. Amer. Soc. Civ. Engs., 7, 224–235.

Storm of August 16–19, 1879. Rainfall recorded at Chestnut Hill Reservoir, Boston Water Works, 4.68 inches in ten hours; 6.23 inches in seventy-one hours.

DESMOND FITZ GERALD,
Trans. Amer. Soc. Civ. Engs., **21**, 96, Plate XXIX.

Storm of February 10–14, 1886. Precipitation in this storm estimated to have exceeded 8 inches upon an area of 750 square miles. Rainfall at New London, Connecticut, 6.66 inches in twenty-four hours; 8.93 inches in fifty-eight and one-half hours.

WINSLOW UPTON, *Science*, **7**, 256.

On Stony Brook watershed, near Boston, the rainfall in this storm is estimated to have been 5.86 inches in seventy-nine hours. (See report upon "Prevention of Floods in the Valley of Stony Brook," by J. B. Francis, E. C. Clarke, C. Herschel, Commissioners.)

On the Sudbury River watershed the observed rainfall in the same storm was 4.64 inches in seventy-seven hours.

DESMOND FITZ GERALD,
Trans. Amer. Soc. Civ. Engs., **25**, 254.

On both these watersheds the storm was accompanied by the melting of snow and ice estimated as equivalent to an additional 2 inches of rainfall, and the discharge of the streams rose to an almost unprecedented amount.

Storm of May 30–31, 1889. Very severe over an area of some 20,000 square miles, lying mainly in Pennsylvania. Rainfall believed to have been 6 or 8 inches on South Fork watershed. At Grampian Hills, adjacent to South Fork basin, the observed rainfall was 8.60 inches in thirty-one hours. Near the summit of the Alleghany Mountains the rainfall is reported to have been from 8 to 10 inches in from eighteen to thirty-six hours. (See report of Committee on "Cause of Failure of South Fork Dam," *Trans. Amer. Soc. Civ. Engs.*, **24**, 446, 447.)

Storm of July 29–30, 1878. Rainfall recorded at Washington, D.C., 5.80 inches in nineteen hours. *Records of United States Signal Service.*

August 5, 1843. At sundry points in Pennsylvania, rainfall reported at 13 inches in three hours, 16 inches in three hours, and 10 inches in two hours, respectively.

American Weather, by Gen. A. W. Greely.

September 29, 1882. At Mayport, Florida, 13.7 inches of rainfall in twenty-four hours.

American Weather.

September 21–23, 1886. At Brownsville, Texas, rainfall of 23.14 inches in sixty-four and one-half hours. *American Weather.*

Recorded in 1884, at St. Louis. Rainfall of 5.05 inches in one and one-fourth hours; 7.55 inches in twenty-nine hours.

E. KUICHLING,
Trans. Amer. Soc. Civ. Engs., **20**, 12.

February 21–22, 1891. Rainfall recorded at Bear Valley reservoir, California, 17 inches in twenty-four hours; 19 inches in twenty-six hours. At Cuyamaca dam, forty miles east of San Diego, the rainfall was 23.4 inches in fifty-four hours, of which 13 inches fell in thirteen hours, and 7 inches in ten hours. *Engineering News*, March 28 and April 18, 1891.

The rate of flow caused by a storm further depends upon:

(*a*) Duration of storm.

(*b*) Extent of watershed.

(*c*) Sundry physical features of watershed, such as steepness or imperviousness of surface.

(*d*) Sundry accidental conditions, such as saturation of the ground; presence of a body of snow; or a close succession of storms.

Shortly after the beginning of a storm the flow at the outlet of the watershed will be representative of the rainfall upon the immediately adjacent area only. The area thus participating will increase with the duration of the storm; but the entire area geographically tributary at the outlet will be effectively so only after a certain time, varying with each watershed. It may require six hours for rain from some parts of the periphery to reach the outlet, and if the storm last but one hour it is plain that the entire area of the drainage basin will not be represented in the flow at the latter point, which may be the waste-weir of the reservoir. Phenomenally high rates of precipitation during storms generally continue for but a short time. Thus at Washington, D.C., in the period 1871–1887, fourteen storms were recorded, which furnished one inch or more of rain in one hour; but although in certain storms the *rate* of precipitation rose to 6 inches, 8 inches, and even nearly to 10 inches per hour, such intensities were of short duration. This is shown by the fact that in no one of the storms did the actual depth of rain falling in a continuous period of one hour exceed 1.8 inches. The highest *rate* was reached in a shower lasting but six minutes, a time far too brief for concentration of the discharge from a natural surface of any considerable size.

In spite of the high rate of precipitation often attained in brief showers, it will in fact be found that the maximum rate of flow from most natural watersheds results, not from such downpourings, but from prolonged heavy storms of one or two days' duration, in which, very likely, no remarkable intensity of rainfall is at any time reached. It will therefore generally be of more value to consider for how much rain falling in a period of say six, or twelve, or twenty-four hours, provision should be made, than to consider the high intensities sometimes recorded for a fraction of an hour. A consideration of the latter is more profitable in connection with problems of sewer discharge, which is quick to respond to rainfall upon the impervious surfaces of cities.

Even if the rainfall were uniform in intensity over the entire drainage basin, and from beginning to end of the storm, the size of the basin relatively to the duration of the rain would be important, as has

been pointed out, in respect to concentration of discharge at the outlet. The absolute size of the drainage basin may be of further importance, since in fact the intensity of rainfall is not uniform over the whole area, the maximum often being confined to a comparatively small portion. Thus, if the given watershed were of large size, it might be raining heavily upon one part and lightly or not at all upon the rest; in which case the resulting discharge at the outlet would be moderate in rate, as averaged for the entire area. The chances are, therefore, general physical features continuing the same, that the rate of maximum discharge will increase as the size of drainage area diminishes. The larger rivers of New England, with watersheds of several thousands of square miles each, the Connecticut and the Merrimack, for example, show maximum rates of discharge not exceeding 20 or 25 cubic feet per second per square mile of basin; while it is tolerably easy to find examples of streams in the same region draining not more than one hundred square miles each, whose flood discharge rises to 40, 50, or even a larger number of cubic feet per second per square mile.

The following memorandum, from a paper by Mr. James B. Francis,[1] shows the estimated areas over which specified depths of rain fell during the remarkable storm which visited New England, October 3 and 4, 1869:

6	inches	or	more,	24,431	square miles.
7	"	"		9,602	" "
8	"	"		1,824	" "
9	"	"		1,046	" "
10	"	"		519	" "
11	"	"		179	" "

A similar estimate, as below, has been made by Prof. Winslow Upton,[2] as to the distribution of rainfall in the great storm of February 10–14, 1886, which was most severe over Rhode Island:

Between	4	and	5	inches,	2,750	square miles (land surface).
"	5	"	6	"	1,850	" " "
"	6	"	7	"	1,500	" " "
"	7	"	8	"	750	" " "
	Over		8	"	750	" " "

[1] Transactions of American Society of Civil Engineers, 7, 234.

[2] Science, 7, 254.

Mr. Emil Kuichling estimates from personal observation that for brief showers, lasting say a quarter of an hour, the least area covered by the heaviest precipitation is probably half a dozen square miles.[1] From such figures as have been given it appears that substantially the maximum intensity of rainfall in a storm is likely to prevail over at least as large an area as is commonly tributary to a storage reservoir.

It is further evident that the maximum rate of flow will be largely influenced by physical characteristics of the watershed. Steep slopes, by shedding quickly the rain to the water-courses, not only limit its chance to percolate into the ground, but have also the same influence as increased duration of storm, in rendering all parts of the drainage area effectively tributary at the outlet. Imperviousness of surface, whether due to rock exposures, as in the mountains; or to the hammering of the soil by the hoofs of animals, as on the Western prairies; or to other conditions, has a marked effect upon discharge. This can readily be appreciated by comparing the behavior of a mountain torrent (although in that case also influenced by steepness of slopes), or that of a Colorado prairie stream, with that of a stream draining the sand hills of the South Atlantic or Gulf coast. The presence of woods, swamps, lakes, ponds, or artificial reservoirs, all of which tend to hold back the rain delivered to them, acts powerfully to equalize the flow of streams and to modify flood discharge.

The various features that have been mentioned all have a bearing upon maximum discharge; but their influence is not more important than that of certain accidental conditions other than the rainfall itself, such as the degree of saturation of the soil, or the presence of a large volume of melting snow at the time of a storm, for example. It is the favorable combination of accidental with fixed and permanent conditions that seems likely to cause remarkable floods. The heaviest rainfall does not necessarily, of itself, produce the highest discharge, as is strikingly shown below in the case of the West Branch of the Croton River. (J. James R. Croes, Transactions of American Society of Civil Engineers, 3, 78.)

[1] Transactions of American Society of Civil Engineers, 20, 13.

DISCHARGE OF WEST BRANCH OF CROTON RIVER.

Date of Storm.	Rainfall Inches.	Accompanying greatest discharge of stream. Cubic feet per second.	
Oct. 3–5, 1869.	5.23 in 60 hours.	350	Storm occurred after protracted drought.
Aug. 2, 1867.	1.96 in 17 hours.	93	August 1, before the storm, the discharge was only about 8 cubic feet per second.
Aug. 16, 1867.	3.38 in 15 hours.	676	Frequent rains, some of them heavy, in previous two weeks.
Feb. 18, 1870.	2.41 in 28 hours.	970	Storm accompanied by rise of temperature and rapid melting of snow. The actual maximum discharge was not measured, but the figures given are Mr. Croes' estimate.

If rain were to fall for a considerable length of time upon an absolutely impervious surface, the resulting discharge from the area would be substantially the same in rate as the precipitation. Indeed, if the surface were first covered with snow, a warm rain might result in a discharge at a rate much greater than that of the precipitation. In fact, in the northern part of the United States the discharge from a given watershed for an entire spring month is often much greater than the entire precipitation for the same time. Thus, for March, 1877, although the rainfall upon the basin was abnormally large, the discharge of the Sudbury River for the month was three per cent. in excess of the rainfall; and there is seldom a year in which, for some of the months, the discharge of this river does not much exceed the contemporary precipitation, rising in some cases to between two and three times the latter.[1] These figures deal, however, with months, and not with the periods of single storms; and the writer is not aware that in any recorded instance the rate of flow from a natural drainage area of any considerable size has risen above, or even nearly as high as, the maximum rate of rainfall for the locality.

[1] *The Flow of the Sudbury River.* Transactions of American Society of Civil Engineers, 10, 232–234.

Various influences have here been presented as affecting maximum flow; yet to assign to them definite values, which shall reduce the whole question to a simple problem, admitting of exact solution, is not practicable with the meager data available as to the actual flow of streams and the attending conditions. It has been seen that in a general way the maximum flow is dependent upon rainfall and upon drainage area; but the details of the relationship are so disguised and so variable that it cannot be nicely traced. In estimating the probable maximum discharge for a given case, the engineer should determine the area of watershed. He may then assume a flow, in inches on watershed, or in cubic feet per second per square mile, founding his assumption upon the maximum known yield of other streams. The principles which have been brought forward should find their application in suiting the assumption to the peculiar conditions of the case in hand. The provision for storm water should be a generous one, for upon it may depend the very existence of the dam and reservoir, while the outlay involved in a suitable waste-way is likely to be, at the most, small relatively to the cost of the works as a whole.

So far as recorded measurements of flow are to be found, there is little to indicate for any of the streams of the North Atlantic slope, and for such drainage areas as we are considering, a greater rate of flood discharge than 6 or 8 inches per twenty-four hours upon the tributary watershed (160 to 215 cubic feet per second per square mile). The Sudbury River, draining what is described as a "fair average New England watershed," some seventy-five square miles in extent, has shown a maximum discharge at the rate of about 1.65 inches per twenty-four hours from the watershed (about 44 cubic feet per second per square mile). In the construction of reservoirs in the basin of this river, for the water supply of Boston, waste-ways are proportioned to discharge the equivalent of 6 inches per twenty-four hours from the tributary watershed, a provision which certainly appears liberal. The same assumption, of 6 inches per twenty-four hours, is stated by Mr. Alphonse Fteley, Chief Engineer of the Croton Aqueduct Commission, to be uniformly employed by him in providing for maximum discharge from watersheds of not more than one hundred square miles, the rate being modified for larger areas.

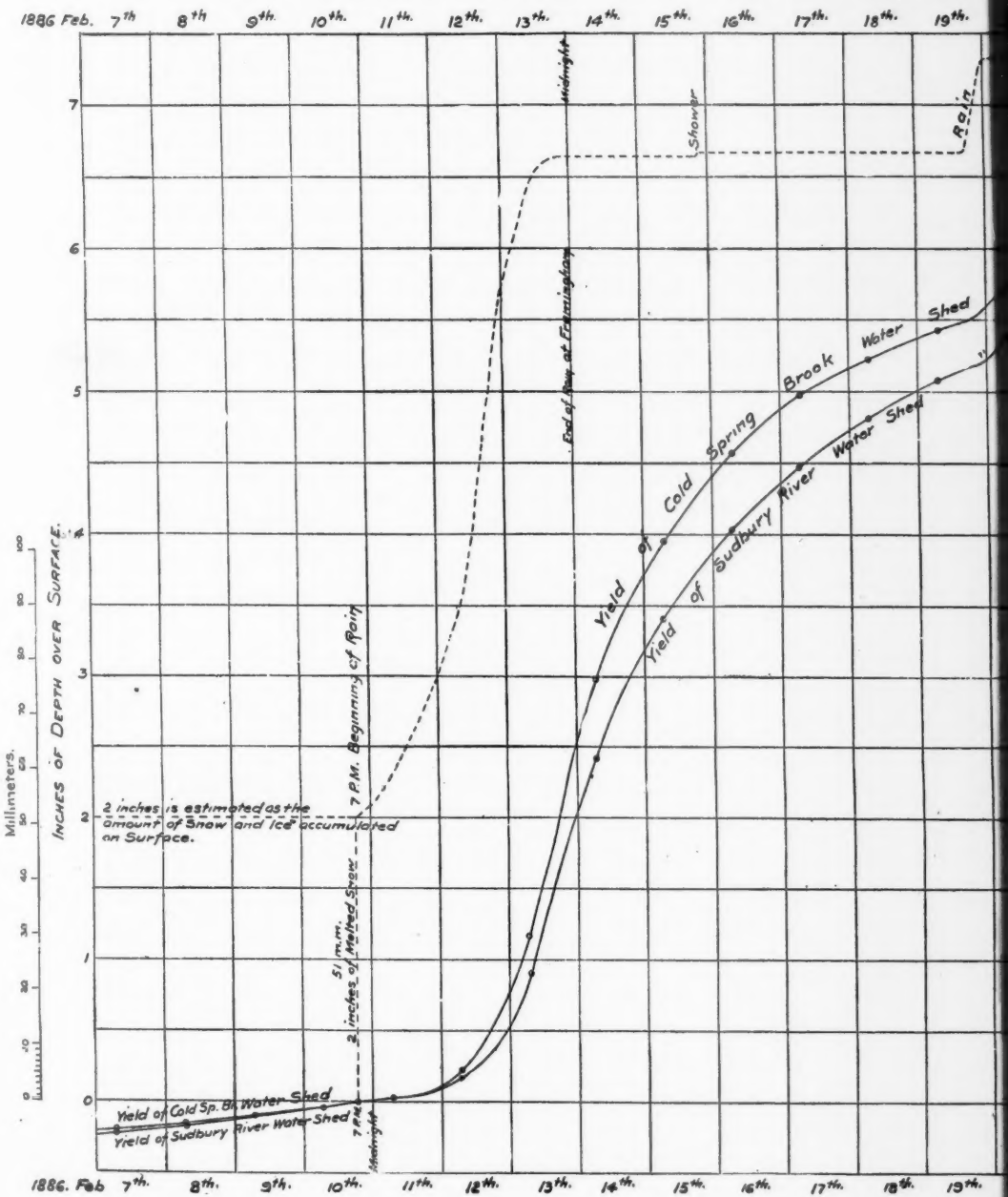
Through the courtesy of Mr. Desmond Fitz Gerald, in charge of the Western Division of the Boston Water Works, the writer is permitted to reprint from the "Transactions of the American Society

of Civil Engineers" for September, 1891, the accompanying diagram, which shows in a most interesting way the history of the flow of the Sudbury River, and its affluent, Cold Spring Brook, during the storm of February 10–13, 1886, in which the discharge was at the highest rate ever observed for those streams. The diagram will in the main explain itself. It will be noticed that ordinates to the curves show cumulative rainfall and discharge from the beginning of the storm, while the slope of either curve at any point indicates the rate of precipitation or of discharge, as the case may be. Data of the character and extent of those which have been collected for the Sudbury River are almost without parallel, and for them every civil engineer and every community concerned with hydraulic works is under obligation.

In the table that follows are presented data, collected from a variety of sources, showing the observed flood discharge of sundry streams, mainly with small tributary drainage areas. Doubtless in many of these cases the discharge was not determined with a high degree of accuracy, and it may be that more extended observations would have shown for some of the streams higher figures than are here given. Nevertheless, the results are the best that are at command, and are believed to be sufficiently accurate to make them valuable for reference.

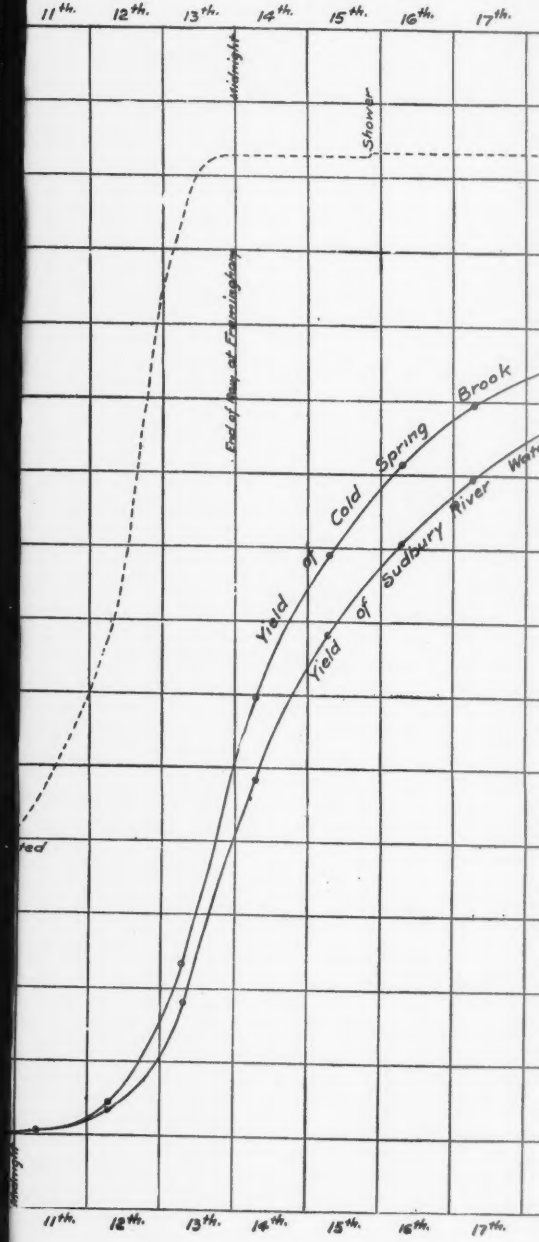
FRESHET OF FEBRUARY 10TH-13TH 1886.

Cumulative Diagram of Simultaneous Precipitation and Yield of Sudbury River a


1886 Feb. 7th 8th 9th. 10th. 11th. 12th. 13th. 14th. 15th. 16th. 17th. 18th. 19th.
Midnight
Shower
Rain
End of Rain at Framingham
Yield of Cold Spring Brook Water Shed
Yield of Sudbury River Water Shed
7 P.M. Beginning of Rain
2 inches is estimated as the amount of Snow and Ice accumulated on Surface.
51 m.m. 2 inches of Melted Snow
Millimeters.
INCHES OF DEPTH OVER SURFACE.
Yield of Cold Sp. Br. Water Shed
Yield of Sudbury River Water Shed
7 P.M.
Midnight
1886. Feb 7th. 8th. 9th. 10th. 11th. 12th. 13th. 14th. 15th. 16th. 17th. 18th. 19th.

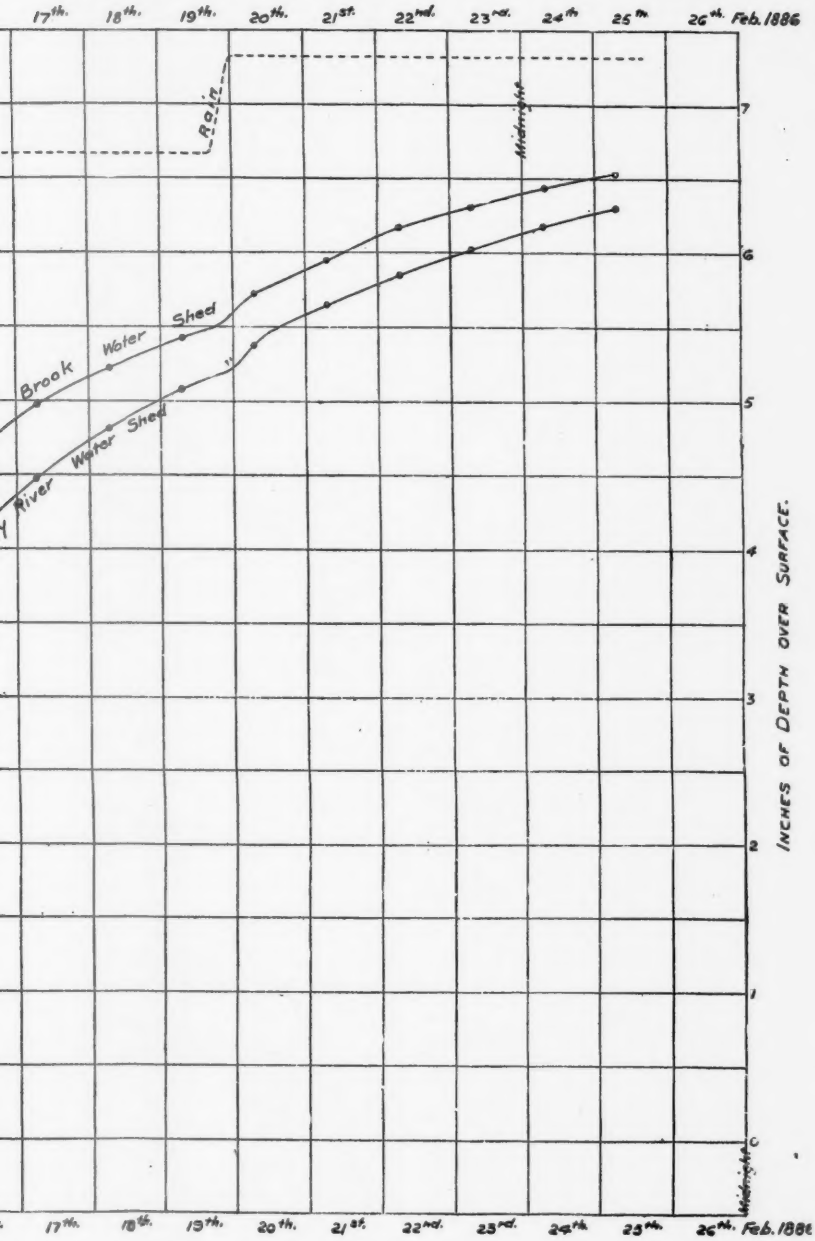
FRESHET OF FEBRUARY 10

multaneous Precipitation and Yield of Sudb



RUARY 10TH–13TH 1886.

of Sudbury River and Cold Spring Brook Water Sheds.



Stream.	Locality.	Drainage area. Sq. miles.	Flood discharge. Cu. feet per sec.	Equivalent in cubic feet per sec. per sq. mile.	Equivalent in inches per twenty-four hours on watershed.	Remarks.
Cold Spring Brook .	Massachusetts . . .	6.43	312	48.4	1.80	February, 1886. Tributary of Sudbury River. D. Fitz Gerald, *Trans. Amer. Soc. Civ. Engs.*, **25**, 257.
South Branch . . .	New York	7.8	577	73.9	2.75	1869. N. Y. State Counsel.*
Woodhull Reservoir	New York	9.4	731	77.8	2.89	1869. N. Y. State Counsel.*
Woodhead Reservoir	Manchester Water-Works, England .	11. ±	3,520 ±	320	11.9	J. F. Bateman.* *Minutes of Proceedings, Institution of Civ. Engs.*, **22**, 362.
W. Branch, Croton .	New York	20.37	1,109	54.4 †	2.02	January 7, 1874. J. J. R. Croes.
Mill River	Massachusetts . . .	58	899 **	15.5	0.58	
Flat River	Rhode Island . . .	61	7,375	120.9	4.50	J. H. Shedd.* Freshet of March, 1843. Due to rain and melted snow.
Sudbury River . .	Massachusetts . . .	74.66	3,305	44.3	1.65	February 13, 1886. Average New England watershed, 38% wooded. D. Fitz Gerald. *Trans. Amer. Soc. Civ. Engs.*, **25**, 257.
Park River	Connecticut . . .	76	2,308 **	30.4	1.13	Drains an open and moderately hilly district. No important reservoirs.
Rock Creek . . .	Georgetown, D.C. .	77.5	9,790 ††	126.3	4.70	W. R. Hutton.
Hockanum River .	Connecticut . . .	79	6,167 **	78.1	2.91	Important reservoir in upper waters.
Nashua River . . .	Massachusetts . . .	109	11,400	104.6	3.89	E. Sawyer.* Valleys narrow, with steep slopes. Two important reservoirs and numerous small mill ponds.

RECORDS OF FLOOD DISCHARGE OF STREAMS — *continued.*

Stream.	Locality.	Drainage area. Sq. miles.	Flood discharge. Cu. feet per sec.	Equivalent in cubic feet per sec. per sq. mile.	Equivalent in inches per twenty-four hours on watershed.	Remarks.
Assabet River . . .	Massachusetts . . .	115	2,184	19.0	0.71	1865. E. Sawyer.*
Scantic River . . .	Connecticut . . .	118	6,116 **	51.8	1.93	Watershed open and moderately hilly.
Pawtuxet River . .	Rhode Island . . .	190	10,800	56.9	2.12	1867. J. H. Shedd.*
Fish Creek	Saratoga Co., N.Y. .	236	1,964	8.3	0.31	1872. E. Sawyer.*
Croton River . . .	New York	338.82	25,367	74.9 †	2.79	April 20, 1854. J. J. R. Croes.
Westfield River . .	Salmon Falls, Mass. .	350	53,000	151.4	5.63	December 10, 1878. Discharge as estimated by Hiram F. Mills. See his report to town of Westfield. Freshet due to a storm in which the rainfall was some three inches, accompanied by the melting of six or eight inches of snow already on the ground. Watershed hilly, mountainous in part, well wooded, but not largely supplied with lakes or reservoirs.
Concord River . .	Lowell, Mass. . . .	352	4,450	12.6 †	0.47	March 30, 1876. Maximum of gaugings for three years only. C. Herschel.
Blackstone River .	Rhode Island . . .	480	4,250	8.9	0.33	1871. E. Sawyer.*
Farmington River .	Connecticut . . .	584	24,375 **	41.7	1.55	Watershed hilly and well wooded. Several important reservoirs.
Chicopee River . .	Massachusetts . .	706	9,635 **	13.7	0.51	Watershed in part hilly, with steep, wooded slopes; and in considerable part also open, moderately undulating, and with extensive marshes.

Potomac River . .	Cumberland, Md. . .	920	17,900 ††	19.5	0.72	November, 1877. Discharge approximate. W. R. Hutton. Watershed wooded, with narrow valleys and steep slopes.
Passaic River . . .	Dundee, N.J. . . .	981	19,944	20.23 †	0.76	December 12, 1878.
Merrimack River .	Lawrence, Mass. . .	4,136	96 780	23.40 †	0.87	1852. C. Herschel. Watershed wooded, and contains lakes and artificial reservoirs.
Delaware River . .	Lambertville, N.J. .	6,500 ±	350,000 ±	53.8	2.00	Ashbel Welsh. *Trans. Amer. Soc. Civ. Engs.*, 10, 241. Watershed hilly and rolling, well wooded, many lakes.
Connecticut River .	Holyoke, Mass. . .	8,006	168,918	21.1	0.79	Watershed hilly, and in part mountainous, well wooded, and contains many lakes and artificial reservoirs. Discharge given for freshet of May, 1854. T. G. Ellis, *Report of Chief of Engs.*, U. S. A., 1878, Appendix B, 14, p. 108.
Kanawha River . .	Charleston Pool, Va.	8,900	118,291 ††	13.2	0.49	August 3,1875. Scott. Watershed mountainous and wooded. No lakes.
Potomac River . .	Georgetown, D.C. .	11,124	175,000 ††	15.7	0.59	Spring of 1852. Discharge approximate. W. R. Hutton.
Mississippi River .	Columbus, Ky. . .	930,540	1,475,000	1.59	0.059	June 16–22, 1858. Humphreys and Abbot. *Report on the Mississippi River*, p. 181.

* Transactions American Society of Civil Engineers, 4, 301.

** Assumed maximum discharge. T. G. Ellis, Report of Chief of Engineers, U. S. A., 1878, Appendix B, 14, p. 55.

† Report of J. J. R. Croes and Geo. W. Howell to Newark Aqueduct Board, 1879.

†† Transactions American Society of Civil Engineers, 10, 242.

SOME RESULTS OF SANITARY LEGISLATION IN ENGLAND SINCE 1875.[1]

By GARY N. CALKINS, S.B.

THE *Revue Scientifique* for April, 1891, contains a review of an article by M. H. Monod on the results of sanitary legislation in England. This article is entitled *Les résultats des mesures sanitaires en Angleterre depuis 1875*, and advocates the extension of sanitary measures in France, where the diminution in population has lately caused much concern.

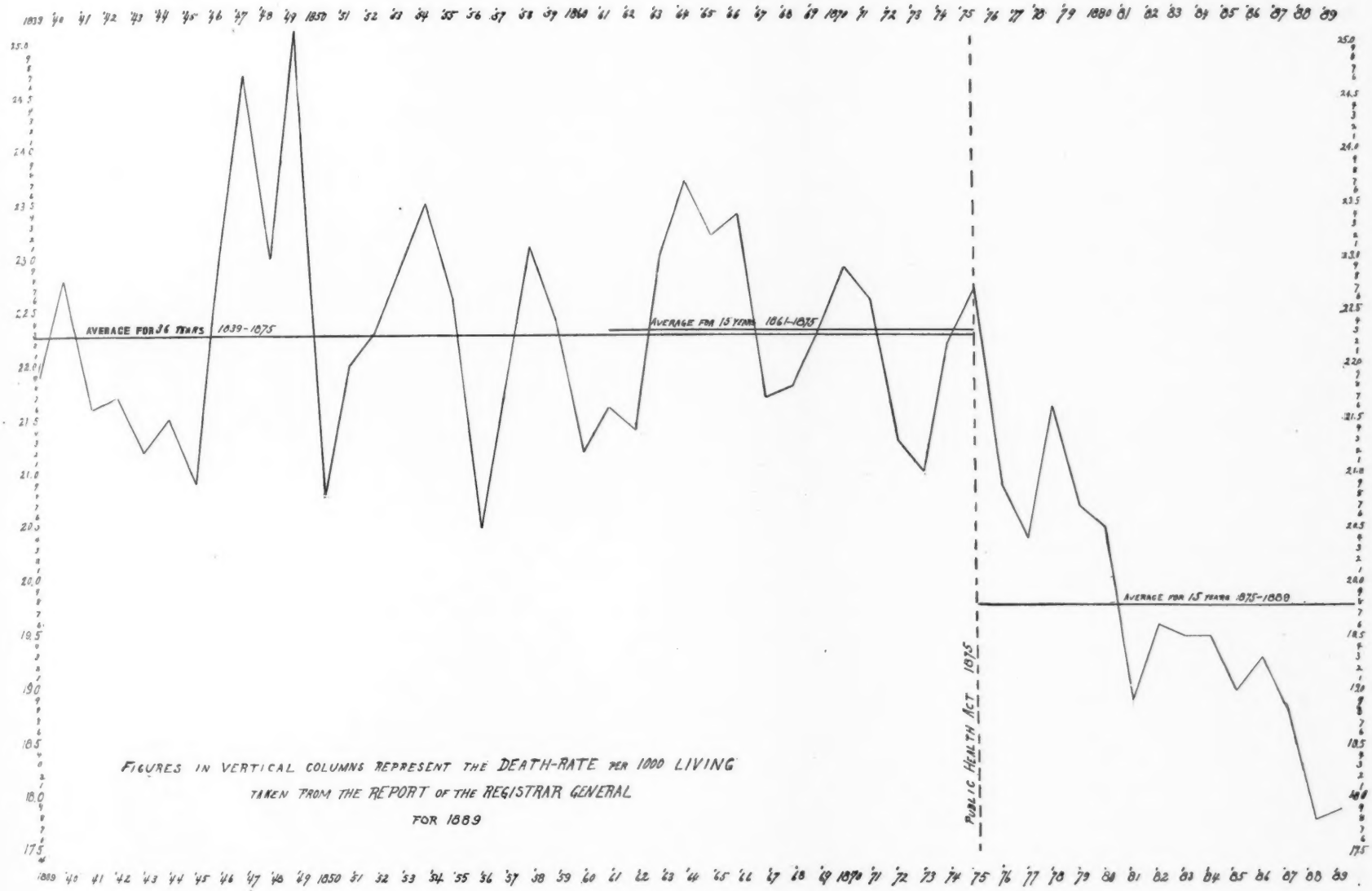
In 1875 a general law was passed in England for the protection of the public health. This was known as the Public Health Act, and since this Act was passed the death rate in England has decreased for all diseases which owe their origin and growth to defective drainage and impure water supplies. Typhoid fever is such a disease, and the diminution of 57 per cent. in the death rate from this malady is undoubtedly the greatest triumph for sanitary reformers.

The cost of sanitary improvements up to 1890 was about $583,500,000, or a mean annual expense of about $42,000,000, and the immediate effect was a marked decrease in the number of deaths, as the curve indicates.

During the ten years from 1866 to 1875 the average annual mortality was 22.19 per 1,000 inhabitants living, as the following figures show:

Year.	Rate.	Year.	Rate.
1866	23.4	1871	22.6
1867	21.7	1872	21.3
1868	21.8	1873	21.0
1869	22 3	1874	22.2
1870	22.9	1875	22.7

[1] Republished, with additions, from the Quarterly Publication of the American Statistical Association, 2, 297.


AVERAGE FOR 36 YEARS 1839-1875
AVERAGE FOR 13 YEARS 1861-1875
AVERAGE FOR 15 YEARS 1875-1889
PUBLIC HEALTH ACT 1875
FIGURES IN VERTICAL COLUMNS REPRESENT THE DEATH-RATE PER 1000 LIVING
TAKEN FROM THE REPORT OF THE REGISTRAR GENERAL
FOR 1889

From 1838, the first year that registration was taken in a careful manner, to 1865, the average annual rate was about 22.35 per 1,000 living. It is sufficiently accurate to state that, for the period 1838 to 1875, the average death rate remained about the same.

But for the ten years of the period 1880 to 1889 the average falls to 19.08; and it is interesting to note that the curve representing the rate since 1875 does not show the great fluctuations which are characteristic of the period before that time. This is probably due to the few epidemics in the last fifteen years, that of 1878 being the most noticeable, when there was a large infant mortality from diarrhœa and whooping cough. The great rise in 1849 and again in 1854 was caused largely by an epidemic of cholera. In 1863, 1864, 1865, and 1866, typhoid fever kept the death rate up to an average of 24.31 per 1,000. Since 1878 the rate is almost uniformly descending, as the following figures show:

Year.	Rate.	Year.	Rate.
1879	20.7	1885	19.0
1880	20.5	1886	19.3
1881	18.9	1887	18.8
1882	19.6	1888	17.8
1883	19.5	1889	17.9
1884	19.5	. . .	. . .

It seems justifiable to assign as the cause of this diminution in the death rate the operation of the Public Health Act and the execution of duties such as drainage, inspection of water supplies, vaccination, and others, which are becoming better and better understood.

Mr. Farr, in his *Vital Statistics*, estimates the value of human life in England to be about $770 a head. "The minimum value of the United Kingdom, men, women, and children, is £159 ($770.36) a head; that is the value inherent in them as a productive, money-earning race." (W. Farr, *Vital Statistics*, page 61.)

The economic value to England of improvements which will check certain diseases, such as typhoid fever, is obvious; for this complaint is especially virulent to man in the adult age, that is, in the age when he is producing to the best of his ability. From the registration sta-

tistics we can calculate the value of the expense to England of the vast sums which she has given out for the protection of the public health.

If we suppose, which is allowable if other things remain the same, that the mortality for each year of the period 1880 to 1889 had been the same as the average annual death rate for the period 1866 to 1875, then, comparing this average with the true rate, we find the number of lives saved has been :[1]

Year.	Lives Saved.	Year.	Lives Saved.
1880	43,457	1885	87,722
1881	85,743	1886	80,546
1882	68,412	1887	95,758
1883	72,013	1888	125,680
1884	72,996	1889	124,477

The total gives a gain of 856,804 lives saved, and this, according to Mr. Farr's estimate, represents a social capital of more than $650,000,000. Thus in ten years the country has more than regained the sum that was spent for sanitary improvements in the fifteen years; and in this calculation nothing figures for maladies avoided; nothing for that which cannot be expressed by figures — spared grief, better health, and happier life.

Mr. Noel A. Humphreys, in an article published in the *Journal of the Statistical Society*, has proved that, if the death rate should continue as low as it was from 1876 to 1880, the duration of the average life of men would be prolonged two years; that of women more than three years as compared with the English life tables; and that 70 per cent. of men and 65 per cent. of the women concerned would be from twenty to sixty years of age, or, in other words, that the extra life would be added to the most productive age. (Longstaff, *Studies in Statistics*, page 226.)

The diminution of mortality is not observed in all forms of disease, and the relative importance of sanitary measures for certain diseases is shown in the following table. The mortality from zymotic diseases

[1] I have found mistakes in the *Revue d'hygiène* in the calculation of these figures, and have therefore re-computed them from the Report of the Registrar General for 1889.

from 1861 to 1870 was 42.54 per 10,000 living, and this was reduced to 24.52 in the period from 1880 to 1889, thus diminishing 18.02 per 10,000 inhabitants. This diminution was distributed in the following order:

Measles	0.02	Diarrhœa, dysentery	2.56
Diphtheria	0.33	Scarlet fever	5.92
Whooping cough	0.78	Typhoid fever	6.36
Cholera	0.91		
Small pox	1.14	Total	18.02

Thus, while the number of deaths from typhoid fever and scarlet fever has decreased notably, measles, diphtheria, and whooping cough have apparently almost completely escaped the influence of sanitary measures.

Consumption has equally diminished in England in these last years. The mortality from this cause was, in the years 1861 to 1870, 24.89 per 10,000 living. For the period 1880 to 1889 it fell to 17.36, a difference of 7.53 per 10,000.

Let us see in just what manner England has gained by these sanitary improvements; at what period or age life has been saved, and whether the average length of life has been affected. The reports of the Registrar General furnish the necessary data.

In studying the figures for the decade 1871 to 1880 it will be seen that the death rate is unequally distributed for the period of the earlier years, but rises for the later years, for men after thirty-five, for women after forty-five. This is shown by an instructive table taken from Longstaff's *Studies in Statistics*, and printed on pages 333 and 334. This shows the death rates for males and females from all causes per 1,000 living at various groups of ages. The average death rate for fifty years is printed in heavy type, as are also the figures for the decade in which the maximum death rate at each age was reached. The last row of figures indicates the rise or fall per cent. of the average death rate for the last decade above or below the average for fifty years.

From this table we see that for the early periods of life the death rate has decreased. In the first decennium, for example, the maximum rate for males between the ages of five and ten was 9.23 per 1,000 in the period 1839–48. The average for the fifty years is 7.78, and the fall from this average in the decade 1879 to 1888 was 28.5 per cent.

For the years between ten and fifteen the death rate decreased from the maximum 5.11 in the period 1849–58, to 3.08, and the fall from the average 4.34 for fifty years was 29 per cent. For the years between fifteen and twenty the fall was 26.3 per cent. from the average 6.04. But for the years between thirty-five and forty-five the maximum death rate was, in the decade 1869–78, 13.97, and in the last decade 12.51, and the diminution from the average 13.03 for the fifty years falls only 4 per cent.; while for the years forty-five to fifty-five the average rate of the last decade *exceeds* the average for the five decennia by 2.6 per cent., for the years fifty-five to sixty-five by 3.3 per cent., and for the years sixty-five to seventy-five it exceeds the average by 3.7 per cent. But above the age of eighty-five years the death rate diminishes from the average by 1.1 per cent. The female mortality is about the same as that for males. It will be observed, however, that the rise per cent. in mortality during the last decade for males, at the age of from forty-five to fifty-five, is 2.6 above the average for fifty years, whereas for females of corresponding age there is a fall in mortality of 2.6 per cent.

DEATHS PER 1,000 LIVING. MALES.[1]

Periods.	All Ages.	0–5	5–10	10–15	15–20	20–25	25–35	35–45	45–55	55–65	65–75	75–85	85–
1839–1848	23.05	71.76	**9.23**	5.06	**7.09**	**9.45**	9.80	12.65	17.83	31.43	66.89	147.5	313.1
1849–1858	23.27	72.68	8.86	**5.11**	6.87	9.10	**9.86**	12.80	18.38	31.35	65.88	146.8	306.2
1859–1868	**28.47**	**72.92**	8.07	4.50	6.28	8.55	9.75	13.22	18.90	32.62	66.40	146.3	313.4
1869–1878	23.13	69.96	7.18	3.93	5.53	7.75	9.74	**13.97**	**20.01**	**34.53**	69.29	**149.5**	**322.7**
1879–1888	20.48	60.13	5.56	3.08	4.45	5.88	7.96	12.51	19.40	33.84	**70.24**	147.7	309.5
50 Years.	***22.68***	***69.49***	***7.78***	***4.34***	***6.04***	***8.15***	***9.42***	***13.03***	***18.90***	***32.75***	***67.74***	***147.6***	***313.0***
Fall or rise per cent.	−9.7	−13.5	−28.5	−29.0	−26.3	−27.9	−15.5	−4.0	+2.6	+3.3	+3.7	+0.1	−1.1

[1] Longstaff, *Studies in Statistics*, page 269.

DEATHS PER 1,000 LIVING. FEMALES.[1]

Periods.	All Ages.	0–5	5–10	10–15	15–20	20–25	25–35	35–45	45–55	55–65	65–75	75–85	85–
1839–1848	21.45	61.52	**8.96**	**5.46**	**7.94**	**8.96**	10.29	12 58	15.63	27.94	60 02	**135.1**	**293.3**
1849–1858	**21.66**	63.04	8.71	5.25	7.64	8.87	**10.35**	**12.68**	**15.80**	27.74	59.29	134.5	286.1
1859–1868	21.27	**63.20**	7.83	4.60	6.79	8.11	9.78	12 08	15.51	27.65	58.95	134.0	286.7
1869–1878	20.47	59.91	6.68	3.94	5.77	7.14	8.94	11 79	15.71	**28.49**	60.50	135.0	292.8
1879–1888	18 30	50.83	5.40	3.22	4.62	5.76	7.63	10.67	15.16	28.07	**61.12**	132.8	280.3
50 years.	***20.63***	***59.70***	***7.52***	***4.49***	***6.55***	***7.77***	***9.40***	***11.96***	***15.56***	***27.98***	***59.98***	***134.3***	***287.8***
Fall or rise per cent.	—11.3	—14.9	—28.2	—28.3	—29.5	—25.9	—18.8	—10.8	—2.6	+0.3	+1.9	—1.1	—2.6

[1] Longstaff, *Studies in Statistics*, page 269.

From this table we see that sanitary measures affect the death rate for persons between the ages of one and twenty-five years, and especially during the period of youth from ten to twenty years. Economically this is a great gain; for the 28 per cent. of those who might have died during this period have now tided over the age of non-production, when they were a burden on the state, into a producing age. Had they died at the age of twenty or thereabouts, the country would have gained no recompense for the expense of maintaining them through the non-productive period.

Or, to put this another way.[1] In 1854 a new-born boy might hope to live 39.91 years. But his son, born in 1880, could hope for 41.35 years. Now suppose both attain to the age of forty. In the first case 26.06 more years of life might be expected, but in the second case only 25.30. Again, suppose 1,000,000 male children were born during the period 1838–54, then at the age of fifty-five 409,460 would survive; of 1,000,000 male children born during the period 1871–80, at the same age of fifty-five, 424,677 would survive. But, at the age of seventy-five, 148,076 would survive in the first case, while in the second there would be only 144,960. That is, the death rate among persons of mature age is greater than formerly. This may be due to the fact that, with improved sanitary precautions, more feeble children live to maturity and die before attaining to old age; or perhaps because of the bustle of the nineteenth century, of the wear and tear upon the nervous system due to competition in densely packed cities and towns.

The effect of sanitary improvements is most noticeable upon infant mortality. If we take 858,878 as the mean annual number of births between 1871 and 1880, and compare it with the average annual number of births for the period between 1838 and 1854, we find the difference between the new life tables and the old to be an annual gain of 1,800,047 years,[2] and this alone is a great defense of the sanitary work now going on. How this may tend to weaken the race is not for me to surmise; at all events it is too late to return to Spartan methods of maintaining it perfect.

France, it is affirmed, is sadly in need of such sanitary improvements as have diminished the death rate in England during the last twenty years. At present her population is at a stand-still; but it

[1] Edinburgh Review, January, 1891, Sanitary Progress.

[2] The above figures are taken from Sanitary Progress.

should increase, and unless it does, there is something wrong in the body politic, since all surrounding countries are growing rapidly in population. Whatever the cause may be for such a small number of births per year, whether moral, or physical, or political, they are all remote and obscure, but the death rate is something that can be viewed and compared, and its causes investigated. Dr. Brouardel,[1] in a recent address before the *Académie de Médecine*, called attention to the disproportionate death rate in France from small pox and typhoid fever. In Germany 110 persons are lost annually by small pox, while in France the number is no less than 14,000. Dr. Brouardel attributes this to the rigidity with which vaccination is enforced in Germany, and the laxity in France.

There are 23,000 deaths from typhoid fever per year in France, and this was shown to be largely due to water supply. Thus in Vienna, before pure water was introduced, the death rate was 200 per 100,000 inhabitants; after the introduction of good water it fell to 10.

In Angôulème, after the introduction of pure water, the death rate from typhoid fever was reduced in the proportion of 18 to 0.063. In Amiens, after pure water was introduced, the number of deaths fell from 111 in 10,000 to 7; in Rennes from 43 in 10,000 to 2. Dr. Brouardel affirms that if vaccination and re-vaccination were obligatory in France, and that if towns were compelled to supply pure water, the saving of life would amount to from 25,000 to 30,000 annually, and this would make considerable difference in the population.

[1] London Lancet, January, 1890.

THREE CRITICS: MR. HOWELLS, MR. MOORE, AND MR. WILDE.[1]

By G. R. CARPENTER, Associate Professor of English.

Of the new books in English that have fallen into my hands during 1891, the three that have interested me most are Mr. W. D. Howells's "Criticism and Fiction," Mr. Moore's "Impressions," and Mr. Wilde's "Intentions."[2] They bear a strong resemblance to one another. They are alike in being collections of short pieces, published from time to time in current periodicals, and expressing opinions with which the ordinary reader has become, by hearsay or by actual knowledge, somewhat familiar. They are alike in being, not poems or novels or plays — not, in short, the kind of work usually styled creative — but critical essays, parasitical, as distinguished from strictly original work. They are alike, too, in that popular opinion allows a certain degree of opprobrium to rest upon their authors. The newspapers jest, properly enough, on Mr. Howells's extreme proclivity for realism; occasionally they even — improperly — sneer at it. In America, at least, Mr. Wilde is as often referred to unceremoniously by his first name as his last, and the impression of the majority is to the effect that intellectually he deserves scarcely more respect than the most empty-headed of dandies. Mr. Moore's name is not so familiar to us, but those who know it at all are apt to associate it with somewhat scandalous novels, or with articles that trumpet the fame of some hitherto unknown author, or with random items in the papers to the effect that, in the columns of So-and-So, Mr. Moore, the brilliant young novelist, has made a violent attack on This-and-That. But

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[1] Reprinted by permission from the *Andover Review* for December, 1891.

[2] Criticism and Fiction, by W. D. Howells, Harper & Brothers, 1891. Impressions and Opinions, by George Moore, Charles Scribner's Sons, 1891. Intentions, by Oscar Wilde, Dodd, Mead & Company, 1891.

in spite of whatever prejudicial associations of this sort one may quite casually and innocently have acquired concerning these three writers, the candid reader will find in their books much that is pleasant and instructive. More than that: from the work of men so totally different in their essential characteristics, he may obtain a singularly complete idea of the most important aspects of modern criticism. But this implies a rough analysis of the three books before us.

To judge "Impressions" fairly, one must first frankly rid himself of some surface irritation. Mr. Moore's inveterate habit of throwing all possible emphasis on the pronoun of the first person, his frequent swagger, his reckless grammar, and his indecorous heedlessness in many matters that deserve accuracy, are all displeasing. It is hard to keep one's temper when he insists on telling us what he said to Turgueneff, and not what Turgueneff said to him; when he habitually prefixes his "I told you so" to each account of a dramatic success or failure; or when he asserts complacently his ignorance of facts about which the nearest book of reference would inform him. Such faults we easily pardon in one writing in headlong haste, to meet an immediate exigency. They are not so easily passed over in a volume of collected essays, in which, more than in books of any other sort, it is safe to assume that the writer works with entire leisure, and with the advantage of adequate previous criticism. Unessential faults, however, the earnest reader can bear with. Why should such trifles of detail spoil his real pleasure? For in books, as in life, minor faults are often the inseparable concomitants of major virtues. The honest man may be blunt even to rudeness, or the sensitive man unbearably fastidious. With defects that spring from virtue these frailties of Mr. Moore's, as we shall see, may be charitably classed.

What strikes first the reader of Mr. Moore's criticism is its refreshing provinciality. It is not the parochialism of a small city or a small clique, but the provincialism of Paris and a part of London. In this he betrays his foreign life and training, and illustrates the trend of his genius. His criticism observes strict historical and geographical limits; it is not concerned with Greek art or the Roman decadence, nor does it make capital of those continued references to the Renaissance, or the fundamental unity of the arts, which it is now the fashion, following Mr. Pater, to introduce with a specious languor into the criticism of any subject. It is not concerned with that scholar's will-o'-the-wisp, comparative literature; not, in short, with aught but what

passes in Paris or in the Anglo-French circles in London for current coin. To the wider if vaguer movements of contemporary literature, Mr. Moore turns a deaf ear. He can write ardently about the Théâtre Libre in Paris without a single hint of her younger and healthier sister at Berlin — healthier, in that around her seem to cling more noble and more human ambitions. Russia, indeed, he knows through the novels of Tolstoi and Turgueneff, and whatever else has been reproduced in French. Ibsen, in spite of his enthusiasm, he quite fails to understand, treating his characters as if they were French, and forgetting that in not one of Ibsen's plays could the action be imagined as going on outside of Scandinavian or Anglo-Saxon surroundings. To the current movements in America, in Italy, in Spain, and in Germany, he never so much as alludes. Germany, indeed, is to him a land still barbarous, and in literature capable only of "Faust," lexicons, and fairy stories.

We have in Mr. Moore's criticism, then, provinciality pure and simple, and a literary interest geographically as limited as if it reached only from Boston to Portland. But it is just this circumscription of interest that, particularly in the Latin races, has been productive of such splendid results in literature, and Mr. Moore is so thoroughly French in his style and his sympathies that one would be puzzled to find the Anglo-Saxon in him. He has nothing of the Englishman's reserve, nothing of his strict holding to models and ideals, of his grim, deductive following out of alleged established truth. In its place we find that supple and vivacious curiosity, that never resting desire to see or hear some new thing, which dignifies and vitalizes French interest in all artistic matters, and, behind this real desire to be amused in whatever ways art will amuse one, a deeper feeling that literature is often (not always) a serious instrument for the propagation of vital truths. Mr. Moore's work, indeed, is not unlike that which we associate with the "Figaro." His style has all the "Figaro's" pert jauntiness, its abruptness and seeming incoherence, and, with them, its admirable sense of just how far one can go in the development of any idea; he has its frankness of expression, its familiarity, its odd Latin way of wearing its heart on its sleeve. This single quality of individuality is Mr. Moore's redeeming virtue. I cannot discover that he has any theory about literature, except that what interests and moves him is, *ipso facto*, interesting and moving. Such liberality of taste, even within the limits we have pointed out, is not a common quality;

even when found, it is too often evanescent. But while it lasts — this healthy equipoise of taste and appetite — it is worthy of sincere admiration. We in America especially, who are in literature by far too much inclined to feed on the prehistoric or the exotic rather than on what is, so to speak, the daily bread of literature, may gain a zest from Mr. Moore's example. His taste is discriminating but heartily sympathetic, and, as a rule, carries the reader with it. Naturally he thinks Balzac the novelist one should read first and cling to longest, but he nevertheless appreciates Turgueneff and Zola, while in Verlaine he finds a singular note of distinction. Nowhere is he persistently immoderate; everywhere he gives natural expression to his distinct individuality — an individuality unpedantic and as yet unsurrendered to the guidance of any system of æsthetics save that which is instinctive in him.

Sharply distinct from that individual criticism which, good or bad, forms the basis of Mr. Moore's "Impressions," is the theoretical criticism which we find demonstrated and illustrated in Mr. Oscar Wilde's "Intentions." Before we can discuss this theory, however, we are again forced to pass through a tedious process of acclimatization; for in Mr. Wilde's book we find ourselves at first in a strange country, a "Wonderland" like Alice's, where the truth appears through its logical opposite. It is, perhaps, not too much to say that Mr. Wilde's only medium of communication is the paradox — and a kind of paradox which strongly recalls his Irish origin. Where another advances by logical steps, slowly leading the reader to the comprehension of something he has not known before, at no point perplexing or confusing him, Mr. Wilde chooses another course. His first object is to bewilder. What he states is the exact opposite of the accepted truth. One's eyes grow large with wonder, or one smiles with an easy contempt and declines to give further time to the discussion of a thesis so ridiculous. On the very first page, for instance, of the dialogue, half Platonic, half after Renan, on "The Decay of Lying," we find this: "Enjoy nature! I am glad to say that I have entirely lost that faculty. . . . My own experience is that the more we study Art, the less we care for Nature. What Art reveals to us is Nature's lack of design, her curious crudities, her extraordinary monotony, her absolutely unfinished condition." A statement at first thought so astonishing and almost revolting as this is shortly followed by others scarcely less surprising. Many a man, says Mr. Wilde, who might have made

a good artist, is spoiled by falling into careless habits of accuracy. It is far easier to do a thing than to talk about it. Action, he declares, is something blind, "dependent on external influences, and moved by an impulse of whose nature it is unconscious. It is incomplete in its essence, because limited by accident, and ignorant of its direction, being always at variance with its aim. Its basis is the lack of imagination. It is the last resource of those who do not know how to dream." Statements like these, seemingly so flippant, prejudice the casual reader against Mr. Wilde, especially when his style is so florid that he might almost be accused of following up Irish bulls with Irish blarney. But such faults of taste and of style, disagreeable as they are, I do not hesitate to call unessential. Mr. Wilde is best when, quitting the extravagant paradox and its luscious development, he writes plain prose. But, after all, that is neither here nor there. We do not care so much how an author writes at his worst, provided that, like Mr. Wilde, he has after all something important or interesting to tell us. In this case it is the author's theory of art, or more particularly his theory of literature, that arrests our attention and holds it throughout. Here, as we have said, Mr. Moore differs widely from his two contemporaries. He follows his own strong bent, without seeing the necessity for a system of ethics or of æsthetics. But Mr. Wilde and Mr. Howells have both built theories for themselves. Each system is the exact opposite of the other; if brought before the general reader's tribunal of common sense, each will, I think, be found right, and each wrong. Both are thoroughly interesting.

Mr. Wilde's main thesis is that the decay of the art of lying accounts for the commonplace character of our contemporary literature. Where older authors have given us fiction for facts, those of today give us fact for fiction. They pore studiously over books, and shamelessly work up their subjects. As a result, we have historical novels like German treatises, realistic novels like the reports of a statistical society, and poems bolstered up with foot-notes. Thus many, who have in them the making of splendid liars, fall finally back on personal reminiscences, and feel the need of corroborating their facts. Nor are foreign literatures better off. M. de Maupassant tears away the veil from life, and makes it ridiculous and ineffective. M. Zola aims at nothing else but the reconstruction of life from "human documents" — of low life, with vices and virtues equally

commonplace and uninteresting. M. Bourget displays a fine subtlety in analyses of human nature, forgetting that at bottom we are all alike, and need not to be reminded of it. There are, of course, a few exceptions: Meredith, for instance, who has by deliberate choice made himself a romanticist, following the model of Balzac, who created life and disdained to copy it. But, as a rule, the modern authors are all in the wrong. They choose the wrong subject-matter; they give it the wrong treatment. Art is something very different from what they suppose. It lies outside of our lives, and scarcely touches them — a land of pure beauty, where nature has little or no part. For nature and art are antipathetic; it was nature that gave Wordsworth "Martha Ray" and "Peter Bell;" it was art that gave him the ode and the fine sonnets.

To be sure, life and art have certain relations. "Art begins with abstract decoration, with purely imaginative and pleasurable work, dealing with what is unreal and non-existent. This is the first stage. Then life becomes fascinated with this new wonder, and asks to be admitted into the charmed circle. Art takes life as part of her rough material, re-creates it, and re-fashions it in fresh forms; is absolutely indifferent to fact; invents, imagines, dreams; and keeps between herself and reality the impenetrable barrier of beautiful style, of decorative or ideal treatment. The third stage is when life gets the upper hand, and drives art into the wilderness. This is the true decadence, and it is from this that we are now suffering." What better historical instance can we have than the English drama? The miracle plays were abstract and theological; life scarcely entered into them. "But with the Elizabethan era the drama took on a language full of resonant music and sweet rhythm, made stately by solemn cadence or made delicate by fanciful rhyme, jeweled with wonderful words and enriched with lofty diction. She clothed her children in strange raiment and gave them masks, and at her bidding the antique world rose from its marble tomb. A new Cæsar stalked through the streets of risen Rome, and with purple sail and flute-led oars another Cleopatra passed up the river to Antioch. Old myth and legend and dream took shape and substance. History was entirely rewritten, and there was hardly one of the dramatists who did not recognize that the object of art is not simple truth, but complex beauty. In this they were perfectly right. Art itself is really a form of exaggeration; and selection, which is the very spirit of art, is nothing more than an intensified mode of over-emphasis."

Art, then, according to Mr. Wilde, is the artist's lie, his deliberate feigning of what is unreal. Life gives the artist the clue which he follows, the stuff on which he in modeling bestows the beauty that charms for an hour, a lifetime, or forever. Life, the truth, the reality; art, the lie, the unreality — this is Mr. Wilde's thesis, so brilliantly defended by him. And why not? Is not this "feigning" of the artist indubitably at the very foundations of all branches of literature, — historically, I mean? And are not we, impassioned for that shadow of reality which incites all the better workmen of today, at home or abroad — are not we drifting away from one great side of art, that which is least concerned with life? "The literary spirit, in its full grace and vivacity," says Sainte-Beuve, "consists in being able to interest one's self in that which is delicately pleasing in reading, in that which is otherwise useless for one, in that which is of no practical service in the eyes of the vulgar, in that which pleads no positive cause, in that which is only the ornament, the flower, the immortal superfluity, of society and of life." "Literature," he says elsewhere, "implies leisure, curiosity, disinterestedness, a breadth of taste, even caprice." In this conception of literature Mr. Wilde and Sainte-Beuve agree. And why should we not agree with them? Why should we not take literature as it is? It is and always has been a kind of ballet, as it were, a continual whirling and posing, with dainty music and softly colored lights, and all that is graceful and charming. But if literature is this, is it always this, this and nothing else? To such a question Mr. Howells answers in his "Criticism and Fiction."

The section of Mr. Howells's book which deals with criticism is not the most interesting part of it. On that subject he has already made declaration of his faith that critics are often prejudiced and uninformed persons, who write in unsigned articles more bitterly than they would dare to do over their names. But he has said this with more acerbity of manner than is customary with him; and, by way of rejoinder, some conspicuous critics of the press, forgetting the dignity of their calling, have bombarded him with somewhat offensive epithets. The controversy does not concern us here. Those in America who are most thoroughly interested in literature recognize Mr. Howells as a man of letters with unusually broad scholarship and wide experience, whose opinions, especially on matters of contemporary literature, though often stated with some extravagance, are uniformly worth consideration. It is the "fiction" part of the little vol-

ume which is valuable. In it the author thrusts aside, once for all, the theory which in Mr. Wilde we have just found admirable — that of art for art's sake. "To spin a yarn for the yarn's sake — that is an ideal worthy of a nineteenth century Englishman, doting in forgetfulness of the English masters, and groveling in ignorance of the continental masters; but wholly impossible to an American of Mr. Henry James's modernity. To him it must seem like the lies swapped between men after the ladies have left the table, and they are sinking deeper and deeper into their cups and growing dimmer and dimmer behind their cigars." We have, then, a fundamental opposition. Mr. Wilde laments that the art of the imagination, the art of lying, is decaying. Mr. Howells rejoices that from lies men turn to truth; that from the works of the pure imagination, or of those touched with the mania of emotional romanticism — from Scott with his mediæval ideals, from Thackeray the caricaturist — the main current of our contemporary literature turns in the direction first pointed out by Miss Austen. She was "the first and the last of the English novelists to treat material with entire truthfulness." After her, by fault of the mania for romanticism, the art of fiction visibly declined in England, as on the Continent, until recently there have sprung up in several countries men who have made friends again with nature and the truth, who write of what they see about them, taking the utmost care that their own fancy shall not color or heighten or in any sense idealize — any further than lies necessarily embodied in human nature — the common stuff of life. The art which is the willing lie these men disdain; their duty is to render account of the life about them "with simplicity, naturalness, and honesty." Of such an art not only the critics, but all men in whom humanity dwells, are competent judges. They "need not cast about for the instruction of some who profess to know better, and who browbeat wholesome common sense into the self-distrust that ends in sophistication," even though "they have been taught to compare what they see and what they read, not with the things they have observed and known, but with the things that some other artist or writer has done." The whole basis of criticism is changing. "The time is coming, I hope, when each new author, each new artist, will be considered, not in his proportion to any other author or artist, but in his relation to the human nature, known to us all, which it is his privilege, his high duty, to interpret."

I have let Mr. Howells speak for himself, because the earnestness and good sense of his words cannot fail to carry with them conviction, though they bear in themselves the seed for an opposition equally earnest and equally just. We have, then, three typical critics, the first a free lance in letters, with booty and pleasure throughout the district of his foray; the second and third, dogmatic knights on either side of the silver-golden shield. To change the figure into one which has a real similarity, the æsthetics of literature are not unlike those of eating and drinking. He who is wise will, with Mr. Moore, show a hearty zest for what comes to his table; like Mr. Howells, he will find most pleasure in fare which is simple and natural; and like Mr. Wilde, he will not disdain, at the proper time, a dainty dish.

AN APPARATUS FOR DETERMINING THE LIABILITY OF OILS TO SPONTANEOUS COMBUSTION.

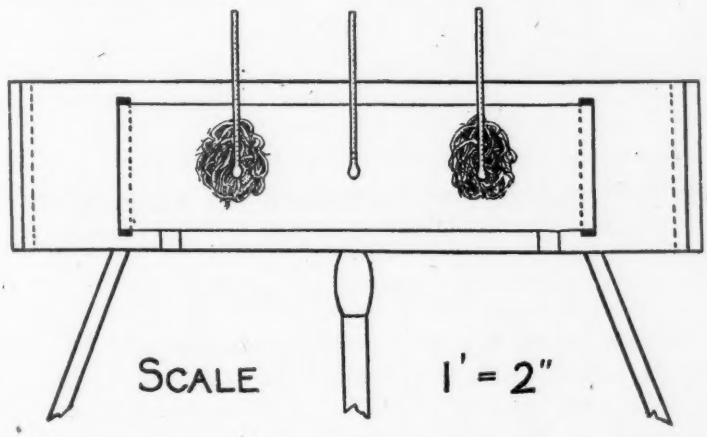
By ELLEN H. RICHARDS.

In 1877–79 the New England Cotton Manufacturers' Association and the Boston Manufacturers' Mutual Fire Insurance Company took up the question of the conditions under which the oxidation of oils, dye-stuffs, etc., in contact with easily inflammable materials, such as cotton in bales, cotton waste, rags, etc., was most likely to occur, with a view to better protecting themselves from loss and to insure a more efficient inspection. The problem was submitted to Professor Ordway, of the Institute of Technology, who suggested a form of laboratory apparatus to experimentally determine the facility with which oils oxidize under favorable conditions. From the results of the investigations of Mr. William B. Allbright, Class of '77, and Mr. Frederick W. Clark, Class of '79, a process was perfected which has been used by me ever since in determining all test cases which have come up in connection with this Insurance Company.[1] The first general conclusions were stated by Professor Ordway, under whose direction the experiments were carried on, in the Proceedings of the New England Cotton Manufacturers' Association, October 30, 1878 (No. 25). The final report was made December 1, 1884, in Special Report No. 18, Boston Manufacturers' Mutual Fire Insurance Company.

The requirements of an apparatus for laboratory use, *i.e.*, not too large, safe for use in an ordinary room, enabling a test to be made in

[1] At that time the only available records of experiments were those of Coleman in Glasgow and Dollfus in Alsace, published in the Bulletin of the Industrial Society of Mulhouse. The description of the apparatus and quantities used in these experiments was rather meager.

a working day of eight hours, easily managed and capable of giving uniform and reliable results, are fulfilled in the one shown in the following sketch:



The apparatus consists of a six-inch wrought-iron tube of thick gas pipe, two feet long, closed at each end by discs of wood; a four-inch tube of thin sheet iron closed at each end with an over-lapping metal cover is contained in this, leaving an inch air-space around it and three inches at the ends. Both tubes are perforated for the insertion of the three thermometers. The apparatus may be conveniently supported upon tripods, and is heated by a Bunsen burner placed midway between the ends. The entire apparatus may be inclosed in a shield if the place is draughty. The diameter of the inner tube permits the use of fifty grams of cotton waste (such as is used to clean machinery), to which is added an equal weight, fifty grams, of the oil to be tested. The oil is evenly distributed by careful manipulation, the waste rolled compactly but not too tightly, and pushed into the end of the tube, which it should fill so that gentle pressure is required to move it. The bulb of the thermometer is now carefully inserted into the middle of this ball. The metal cover, which should fit loosely so as to admit a little air, but affording protection from currents, is put on, and then the outer cover of wood. A blank of unoiled waste is placed under the same conditions in the other end. The thermometer in this blank should not be permitted to rise above 100° or 101° C. at the most. To have it reach this temperature, the middle thermometer must be

kept at about 125°. Since the balls of waste are equidistant from the source of heat, the necessary conditions are fulfilled if there are no draughts to blow the flame or cool one end of the cylinder.

In this apparatus anything which can be finely divided — shavings, leather, skeins of dyed yarn or rolls of dyed cloth — may be readily tested. Paper pulp, boards, and other carbonizible substances may be tested at a higher degree of heat for inflammability. Coal has not been subjected to experiment in it.

The results of greatest practical value obtained in the use of this apparatus have been, first, determining the cause of fires; and second, determining the degree of safety of the various oils used in manufacturing. Mineral oil, as is well known, is not liable to spontaneous combustion; and a certain percentage of animal or vegetable oil may be added to mineral oil without materially increasing the danger under ordinary circumstances. This percentage varies according to the oil; with neat's-foot and first quality lard oil some 50 to 60 per cent. may be used. With cotton-seed, not over 25 per cent. is allowable. The claim so often made for so-called "safe" oils, said to have been changed by special and secret processes of refining so as to be no longer dangerous, is easily exposed by this test.

Massachusetts Institute of Technology,
February, 1892.

BEAUMARCHAIS AND THE LOST MILLION.[1]

By ALPHONSE N. VAN DAELL,
Professor of Modern Languages.

While the Revolutionary struggle lasted, and even, to a certain extent, during Lafayette's life, the French influences which assisted the States in forming a nation were appreciated; but it has often seemed to me that the present tendency in many minds is to belittle the part taken by France, to ignore altogether the powerful current of the eighteenth century, which surely had some action in shaping the feelings of Americans like Paine, Jefferson, Franklin, and their contemporaries. It ought to be an interesting study to compare the Declaration of Independence and the Constitution of the United States with Rousseau's Contrat Social, for example, and mark the parallelisms which are visible even on a very cursory reading.

The building of legends diverging both from truth and justice has been going on, it seems to me, under English influences. Natural as it is that Americans should ever turn with love towards their mother country, to which so many hallowed remembrances must bring them, still it may be necessary to remind some that England is not the whole of the civilized world, and that they must guard against unconscious influences, the strongest of which is undoubtedly the community of language and literature. America has cut the political leading-strings that held her back; she has yet to efface more completely than in the past some of the intellectual influences and national prejudices that were necessarily a part of her early education. Among the prejudices which reign supreme in England is the contempt for foreigners; and I more than suspect that part at least of this prejudice subsists in minds like that of Mr. Stillé when he writes, in his article on "Beaumarchais and the Lost Million," these sneering words: "This letter [of Beaumarchais's], which is of great historical value as showing not

[1] An Address before a private club in Boston, February 19, 1892.

merely the nature of the *disinterested services* of France during the Revolution, but also the plan and motives of Beaumarchais . . ." (The italics are Mr. Stillé's.)

Now nothing in history is better proved than the disinterestedness of the motives which brought to the aid of the American colonies men like Lafayette; nothing more certain than the general aspirations for universal liberty and justice that prompted at that time the masses of the French nation. That the French government, which was surely not identical with the nation, may have perceived advantages of a different nature, and may have used America's hand for striking at the hereditary enemy, is also true beyond a doubt. But this consideration could hardly justify national ingratitude for services actually rendered; and that Beaumarchais did render invaluable service to the American cause at the most trying time is not to be denied.

Mr. Stillé says of him (I quote Mr. Stillé because he, being the most recent of Beaumarchais's accusers, has compounded in his pamphlet all that has been brought against him): " . . . He had been condemned by the Parlement de Paris to an infamous punishment for having produced in one of these lawsuits a receipt or discharge of a debt which that court had pronounced supposititious. . . . "

But the American writer fails to tell us by what intrigues, by what terrible abuse of power, this condemnation was obtained, and how public opinion reversed the decision of the judges, and soon after destroyed the corrupt tribunal that had sold injustice at the beck of the rich and powerful.

Mr. Stillé continues: " . . . To many he seems only a vain, ever active, unscrupulous intriguer, employing without hesitation lying and mystification whenever necessary to accomplish his object, which is assumed to have always been his self advancement and the gratification of an inordinate vanity for making himself talked of. By others he is thought chiefly responsible for the success of two revolutions, that of France . . . and that of America, by the energy which he exhibited in supplying us with arms and clothing for an army of twenty-five thousand men — supplies which we must admit were essential to our military success against Great Britain. . . ."

And a little farther we find the significant sentence: " . . . And for his aid to America in the hour of her sorest need, whatever may have been his motive, or however questionable may have been some of his proceedings, we should never cease to be profoundly grateful."

Let now Mr. Stillé bring forward his accusation. Let us drop the question of Beaumarchais's motives; this is an idle one, since it is difficult enough to establish facts, let alone the inner workings of our hearts. Unless the accuser makes out a very clear case against Beaumarchais, the man who *did* render the service he claimed at some risk of his life and the much greater risk of his fortune and who surely had *not* been the gainer by his transactions, we come inevitably to the conclusion that the odium of quibbling rests with the American Congress. But Beaumarchais's cause need not be left at this point; and in order not to do him the least favor, let Mr. Stillé explain the debated point:

"To this man our forefathers were told, in the early days of the Revolution, to look for succor and safety. The more they heard about him, the more completely did his position seem a mystery and riddle to them. To their sober and practical minds it was hard to conceive of him as a beneficent fairy, who, unsolicited, was willing to send us millions of dollars' worth of the supplies we most needed to carry on the war, and who, as they were told by Arthur Lee, the earliest American Commissioner in Europe, never suggested that he was to be paid for them, but merely hinted that it would be well for Congress to make him certain shipments of tobacco to conceal his transactions from the English. The Americans had no experience of merchants who carried on business in this way. It was not their interest, however, to inquire too closely into the source from which these supplies came, and they were inclined to settle down into the belief that Beaumarchais was a convenient *prête-nom* for the French government, which had so many reasons for encouraging us in our resistance, and so many more to conceal the aid it was giving us in our struggle with Great Britain. Whatever the Secret Committee or the Committee on Commerce of the old Congress may have thought or suspected, they did not, so long as the military supplies were provided in reasonable abundance, inquire with too much curiosity into their source. They accepted them with thankfulness, not doubting that they would discover in due time to whom they were indebted for them. In the mean time they chose to regard them as gifts from the King of France. For more than two years and a half they were under this pleasant delusion. They were confirmed in the opinion expressed by Arthur Lee, by letters written by the American Commissioners in Paris, who told the

Secret Committee in October, 1777, that they had been assured that these supplies were intended as *don gratuit*, or a voluntary gift on the part of the French government. Of course, under these circumstances, they made no effort to pay for them. Beaumarchais, under the name of Roderigue Hortalez & Co., tired of waiting for remittances from this country, sent an agent here in the beginning of the year 1778, who, in a tone very different from the high-flown rhetoric of his master's letters, demanded full payment for all the supplies which had been sent. This demand dispelled all dreams about *don gratuit* on the part of France. After ascertaining from the French government that Beaumarchais, under the name of Hortalez & Co., was really the man they had been dealing with, Congress, in January, 1779, made a partial settlement with his agent, and directed their President to write a letter of thanks and of apology to Beaumarchais for their delay in paying him. He received a large sum on account at that time, but a final settlement was delayed until further information from France reached them."

Let us stop here one moment: Why in the name of common sense, if to the sober and practical minds in Congress it was so hard to conceive of Beaumarchais as a beneficent fairy, if all dreams about the *don gratuit* were dispelled, if the President wrote a letter of thanks and apology to Beaumarchais, why not pay him at once, and why delay until further information should come from France?

But let Mr. Stillé continue: "While the liquidation was slowly going on, the French government, in 1781, entered into a treaty with our Commissioners in Paris, by which it agreed to loan to the United States a certain sum of money. In the preamble to this treaty there was a recapitulation of the sums paid by the French government for our use and account, but regarded as voluntary gifts, prior to the treaty of alliance in 1778, and an explicit statement was made that these sums, amounting to three million of livres, were to be considered as an absolute gift from the French King to us, and we were released from all liability therefor. It was discovered some time afterwards that our Commissioners had been paid by the French government not three millions, but two millions, prior to the treaty. Inquiry was of course made in France as to the person to whom this million had been paid. That government, for reasons which will be explained hereafter, refused to give the name of the person to whom payment

had been made, but it gave the date of the payment, June 10, 1776. The accounting officers of the treasury believed that this particular million was a portion of the secret service money paid by France to Beaumarchais for our use on the 10th of June, 1776, and that it had been expended in the purchase of supplies sent us, and as this million had subsequently been made by the action of the French government a present to us, they felt that Beaumarchais should not be paid twice for the same service, and thus they charged it against him in the settlement of his claim.

"Beaumarchais protested against such a decision. A controversy ensued, which lasted until 1835, in which the questions were, Whence came this 'lost million,' and what had become of it; by whom had it been used, and for what purpose? — questions between ourselves and Beaumarchais in the first instance, and afterwards between the French government and ours, which urged in his name his claim upon us. These questions involved interminable discussions about the rights and obligations which arose out of our secret diplomatic arrangements with France during the Revolution. They were the subject of many conflicting reports from committees of Congress for more than fifty years; two Committees on Claims, two Select Committees, and two Attorney-Generals recommended the payment of the claim, while two Committees on Claims and one Select Committee reported against its payment.

"It seems to me important for the good name of the country, as well as for the truth of history, that these transactions and their motives should be carefully examined and the fullest possible light thrown on them. As a matter of curiosity it is certainly worth knowing what became of this lost million, the origin and destiny of which were so carefully covered up by the French government. But the interest in such a discussion is of a wider and more permanent kind, for it embraces not merely a view of the manner in which military supplies essential to the prosecution of the war of the Revolution were obtained, but how far they were voluntary gifts bestowed upon us by France to secure her own ends in our quarrel, and how far we showed ourselves duly grateful for them. Our government has been branded with something worse than ingratitude — with dishonesty — in its persistent refusal to pay Beaumarchais this 'lost million.' Such an impression was doubtless made on the French government by our course — judging from the voluminous correspondence which was kept up for so

many years between that government and our own — and our conduct was long spoken of as an illustration of the old saying that republics were not only ungrateful, but shifty and tricky in their dealings."

I regret that I cannot review here the whole history of the transactions with Beaumarchais, for with him as an actor the direct documents teem with life and interest. But this would take an amount of time which is not at my disposal.

Let us then confine ourselves strictly to the so-called lost million; that is, to the question whether the million livres-tournois paid Beaumarchais, June 10, 1776 (for he is the person to whom the sum was given), was intended to pay for the ammunition he had sent or was to send to America. Beaumarchais and all the successive agents of the French government have denied it, while the defense for the refusal to pay the sum must needs rest on the affirmative answer.

Beaumarchais's receipt is worded as follows:

"J'ai reçu de M. Duvergier, conformément aux ordres de M. le Comte de Vergennes, en date du 5 courant, que je lui ai remis, la somme d'un million dont je *rendrai compte* à mon dit sieur Comte de Vergennes.

"CARON DE BEAUMARCHAIS.

"Bon pour un million de livres-tournois.
A Paris ce 10 juin, 1776."

The question is one very difficult to solve; that is, it may be forever impossible to find a written proof of the reason why the payment was made. The French government at the time had excellent grounds for not desiring to have its own doings known; and, generally speaking, even in our days of free inquiry, governments do often reserve funds for the disbursement of which they do not wish to give account to outsiders. Now it is a fact of public notoriety that Beaumarchais had been in relation with Count de Vergennes, and before him with King Louis XV, and that he had successfully concluded the several secret missions with which he had been entrusted. Whether he had taken to himself more credit than he was entitled to, is not to the point; and it may not be unnecessary to say that I do not consider Beaumarchais to have been a saint, nor are we to sit in judgment over the relaxed morals of his age.

Let us simply examine the circumstances, for we are reduced to circumstantial evidence; this evidence appears to me so strong, so decidedly favorable to Beaumarchais, that it is hard to understand how candid and unprejudiced minds could hesitate. I believe the case to be this: When the French government, yielding to the representations of Beau-

marchais and other men, resolved, for political reasons of their own, to assist the struggling colonies, then, as Stillé and others acknowledge, in the hour of their sorest need, Count de Vergennes understood very well that the help must be indirect, and that under no circumstances must it become known that France, while at peace with England, had taken an underhand part. He asked Beaumarchais to become his intermediary. Now the latter had an adventurous mind, to be sure; but did it ever occur to Mr. Stillé and his predecessors that the enterprise, judging it from a commercial standpoint, involved a great many risks — many more than a cool-headed business man would ever care to assume? For instance, the States might succumb in the struggle, or even if they triumphed might become insolvent for years to come, might even go into bankruptcy, or payment might be delayed, or they might refuse to accept Beaumarchais's cargoes, or the English might capture his vessels, or his ships might be drawn into naval battles and come out damaged. Not one of these suppositions would have seemed extravagant at the time of Beaumarchais's contract, and in fact the last three were realized. Whatever money Beaumarchais obtained, after the most vexatious delays, was paid not in cash, but in promises payable three years after, and some enterprising financial officers of the States inquired even about the means of not paying the notes when they became due.

Suppose, now, that Count de Vergennes, wishing to induce Beaumarchais to accept all the responsibilities resulting from his actions, had promised him *one million livres-tournois*, not as payment for any special purpose, but simply as an indemnity for any claims Beaumarchais might have against the French government for assuming the risks of business. This sum could truthfully be said to have been spent for the American colonies, and yet could not be considered as a partial payment for the ammunition that Beaumarchais sent over. This theory is, I believe, confirmed by all the facts known to me; it agrees with the repeated affirmations of the French government, of Beaumarchais, with the wording of the treaty between France and the United States, as I shall attempt to prove, and is in contradiction with the testimony of but one single witness, Arthur Lee.

This personage, whom Jared Sparks in his life of Benjamin Franklin (see page 447 and following) calls an uneasy and violent character, jealous and distrustful, causing quarrels and difficulties wherever he goes, is at the bottom of Beaumarchais's troubles. Some idea of his

high self-appreciation may be obtained from his first confidential letter to the Secret Committee of the Congress, dated January 3, 1777: "The politicians of this court are in a kind of trembling hesitation." You could never guess why! "It is," continues Lee, "because the promises made me by the French agent in London have not been entirely kept." Imagine the King of France and his hosts trembling in the presence of Arthur Lee!

In another letter Lee has the audacity to write that M. de Vergennes and his secretary have repeatedly assured him that no return was expected for the cargoes sent by Beaumarchais. And this was after Silas Deane had written to Congress (December 29, 1776) that Beaumarchais has trusted the United States for a considerable amount, and that he confidently expects them to repay him promptly. Lee, for reasons of his own, which I do not pretend to judge, affirmed a thing which he and Silas Deane and Benjamin Franklin knew to be false. This is sufficient to show the animus of the man. A letter of Beaumarchais of December 20, 1777, will serve to explain what he felt, and what was going on:

"Je ne sais si je me flatte, mais je compte sur l'honnêteté, sur l'équité du Congrès comme sur la mienne et la vôtre. Ses députés ici ne sont pas à leur aise, et le besoin rend souvent les hommes peu délicats: voilà comment j'explique l'injustice qu'ils ont essayé de me faire. Je ne désespère pas même de les ramener à moi par la douceur de mes représentations et la fermeté de ma conduite. Il est bien malheureux, mon ami, pour cette cause, que ses intérêts aient été confiés à plusieurs personnes á la fois, un seul eût bien mieux réussi, et pour ce qui me regarde, je dois à M. Deane la justice qu'il est honteux et chagrin à la fois de la conduite de ses collègues avec moi, dont le tort appartient tout entier à M. Lee."

Later on, Lee is the only man whom Beaumarchais refuses to accept as an umpire, and it is Lee who, of all men, is chosen by the Congress to examine his claims.

That the French government must have, as I suggested, guaranteed Beaumarchais against certain losses, is shown by a letter of his to the King, Louis XVI, dated September 11, 1779. Beaumarchais's ships had taken a glorious part in a battle against the English fleet, and there were pressing needs of money which he exposed. The King granted his request for a loan of four hundred thousand livres, which was repaid later on. Beaumarchais was also paid for the actual loss of his merchantmen, which had been scattered in consequence of the battle. This transaction shows, it seems to me, that an agreement must have existed between the French government and Beaumarchais, and the conclusion is very near that the lost million was included. On

January 15, 1779, John Jay, President of the Congress, writes from Philadelphia to present the thanks of his country to Beaumarchais, regrets the misfortune which he has undergone for the defense of the States, and promises prompt payment.

On September 3, 1778, W. Carmichael writes a most friendly letter, in which he assures him of his friendship, and takes sides with him in his struggle against Lee and for Benjamin Franklin. In October, 1779, the Congress sends Beaumarchais two million five hundred and forty-four thousand livres in three-year promises. This was perhaps taking too literally Count de Vergennes's words, who had told the American envoy, three years before, that he need not hurry with the payment of the claims.

There was some afterthought in the minds of Congressmen, for a little later we hear of Robert Morris inquiring of Benjamin Franklin whether there was not some means of avoiding the payment of these promises. And it is astonishing to see Franklin, after being urged by M. Darival for the reimbursement of the French advances, ask again whether Beaumarchais is to be paid. The French officials answer, of course, that they do not know anything about the Beaumarchais claim. In 1781 Silas Deane — who had been on the spot since the first and watched every transaction — having returned to France, finds in his summing up that the States owe Beaumarchais three million six hundred thousand livres. In 1783 a new agent is sent by the United States, Mr. Barclay; he refuses to accept Silas Deane's accounts, and in spite of Beaumarchais's protest, proceeds to revise once more. He reduced the amount admitted by Silas Deane. But the States did not pay anything, and now an incident came up which was to be the cause of the indefinite postponement of the Beaumarchais claim.

The United States were already owing considerable money to France, and still asking for a further loan of six million livres. It was agreed that the whole financial situation was to be cleared up by an exact statement in the contract of all sums previously received, either as a loan or as a gift.

As loans were enumerated eighteen millions; then ten millions obtained in Holland but guaranteed by France, and on which the King of France was paying interest; and finally the new loan of six millions, making together thirty-four millions which were to be refunded.

As gifts, that is to say as sums which the King had used for the Americans but for which he did not claim anything, were enumerated

first, three millions given before 1778, and second, six millions given in 1781. Now the American agents had received only two millions instead of three, as Franklin singularly enough did not remember until 1786. But this very fact proves that neither Americans nor Frenchmen at that time considered that any consequences should be drawn from that declaration, which was meant only as a kind of memorandum of the sums expended, for which the King did not wish to receive reimbursement. Franklin had received two millions directly from the French treasury, and one million through the intermediary of the Fermiers Généraux, on account of which some one hundred and fifty thousand livres had been paid in tobacco. But after inquiry the French government answered that it had nothing to do with that million, but that one of the millions referred to in the second item had been paid to a person not to be named, on June 10, 1776.

Allow me a short parenthesis which will make clear the way in which money matters seem to have been then treated by the United States government, and which goes far to explain the Beaumarchais controversy. Franklin claimed interest on the one hundred and fifty and odd thousand livres paid by the American government, but no mention was made of the interest which was evidently due to the Fermiers Généraux on the eight hundred and fifty thousand livres making up the round million, so that the Americans expected interest on the partial payment of a sum they owed, while not paying themselves any interest on the principal. In a letter of the French Chargé d'Affaires, M. de Marbois, to Count de Vergennes, dated Philadelphia, August 24, 1784, we find the following characteristic appreciation:

"I do not believe M. M. [evidently Mr. Morris, Secretary of the Treasury] capable either of love or aversion for any power, but I have good reason to think that his greed may lead him into most reprehensible irregularities, and that, unless bound by instructions from the Congress, he will care very little to fulfil the obligations of the United States toward His Majesty the King of France."

The French minister refused, on September 16, 1786, to communicate what we know to be Beaumarchais's receipt for the million, or even to name the signer, and the Congress now concluded, first, that Beaumarchais had received the million; second, that this million ought to be refunded by him; third, that the Congress was not bound to pay anything until the mystery had been explained. While this was going on, Beaumarchais, growing desperate, asked at least for

submission of his claims to umpires, being willing even to let the Americans name them all, provided only that Arthur Lee should be excluded.

On the 12th of June, 1787, he wrote to the President of Congress as follows:

"Que voulez-vous, Monsieur, qu'on pense ici du cercle vicieux dans lequel il paraît qu'on s'enveloppe avec moi? Nous ne ferons aucun remboursement à M. de Beaumarchais avant que ses comptes ne soient réglés par nous, et nous ne réglerons point ses comptes pour n'avoir point de remboursement à lui faire! Un peuple devenu puissant et souverain peut bien regarder, dira-t-on, la gratitude comme une vertu de particulier au-dessous de sa politique; mais rien ne dispense un État d'être juste et surtout de payer ses dettes. J'ose espérer, Monsieur, que, touché de l'importance de l'affaire et de la force de mes raisons, vous voudrez bien m'honorer d'une réponse officielle sur le parti auquel l'honorable congrès s'arrêtera, soit de me régler promptement et de solder son règlement, comme un souverain équitable, soit de choisir enfin des arbitres en Europe pour juger les points en débat *d'assurances et de commission*, ainsi que M. Barclay eut l'honneur de vous le proposer lui-même en 1785, soit enfin de m'écrire sans détour que les souverains d'Amérique, oubliant mes services passés, me refusent toute justice: alors j'adopterai le parti le plus convenable à mes intérêts méprisés, à mon honneur blessé, sans sortir du profond respect avec lequel je suis, du congrès général et de vous, Monsieur le président, le très-humble, etc.

"CARON DE BEAUMARCHAIS."

Congress answered by naming Arthur Lee to examine the claim. He found not only that Congress owed Beaumarchais nothing, but that the latter was indebted to the United States for the sum of one million eight hundred thousand livres. It is true that Mr. Alexander Hamilton, revising the account, found, in 1793, that Beaumarchais was a creditor for two million two hundred and eighty thousand livres; but he concluded that it was not advisable to pay the claim until the French government should have been asked again for that now celebrated receipt.

In consequence, Gouverneur Morris requested Buchot, then Minister of Foreign Affairs, on June 21, 1794, for the desired communication; and the latter, actuated probably by the desire of doing the reverse of what had been done during the Ancien Régime, surrendered it. The Congress immediately asked Beaumarchais to prove that the million which he had received was not intended for the United States. The reply followed immediately:

"Je demande qu'il me soit donné acte de la déclaration la plus précise que je fais, que jamais je n'ai reçu du roi Louis XVI, de ses ministres, ni de personne au monde, *ni un million, ni même un seul shilling pour vous être offert en présent;* que tout l'or que j'ai employé pour vous servir, en ami bien zélé, en loyal négociant, et au seul titre d'un commerce équitable, n'a été rassemblé par moi, tant en France qu'en d'autres États de l'Europe, qu'à titre d'association *d'emprunt ou de circulation;* que tous mes créanciers, moins patients envers moi que je n'ai dû l'être envers vous, ne m'ont pas laissé vingt années sans exiger leur compte et leur acquittement, et que s'il *m'en restait quelques-uns à solder, question qui vous est étran-*

gère en qualité de débiteurs, ce ne serait qu'une obligation de plus pour vous de me mettre en état de le faire en vous acquittant envers moi. Quant au contrat de 1783, dont vous m'apprenez l'existence et que j'ai toujours ignoré, je déclare que ce contrat, où je n'ai pas été appelé ni par vous, ni par les ministres de France, m'est absolument étranger, sous quelque point de vue qu'on l'envisage, par cela seul que je n'y ai point été appelé, ce qui était indispensable, si vous deviez, après douze ans, essayer de vous en faire un titre pour éluder ou éloigner mon payement, après avoir épuisé tous les autres."

The last letter which the dying Frenchman wrote to Congress again demanded justice — no longer for him, but for his daughter, and for his widow. These latter claimed over four million, but asked in vain, at least for the sum which Alexander Hamilton had named. Talleyrand writes on the 28th of Germinal XI, and again in 1816 the Duke of Richelieu affirms to Mr. Gallatin, that the one million given to Beaumarchais on June 10, 1776, had nothing to do with the ammunition sent to America.

It is only in 1836, when the twenty-five million controversy and President Jackson's violence showed the French that the United States were not as patient creditors as they themselves had been, that they succeeded in having the Beaumarchais claim (reduced, it is true, to eight hundred thousand livres, a figure much below that which anybody had ever named) included in an international settlement.

It may be asked why I have taken up this subject. Let me tell you at once that it is not for the vain pleasure of complaining of the American nation, nor for the purpose of showing the United States in an unfavorable light. I do not believe that the case of Beaumarchais is unparalleled in the annals of history. I simply wished to call attention to the injustice done, first, because I think there is among the causes that led to it a narrowness of spirit, a kind of race prejudice which is far from dead, if we may judge from Mr. Stillé's book; second, because it teaches a lesson which may be valuable for the future. It is not best to entrust to a large body of men the solution of questions of personal justice, especially when the claimants have no political influence. The average politician is always anxious to have money spent in his district, where it will show his admiring constituents his power and influence. But when the money is to go far away, he is likely to think that he ought to be very economical; he will stop at no quibble; he will depend on the judgment of persons as careless and ignorant as he is himself, and refuse to see the injustice or harm he has done. *He* has done none in any case; it is the majority, and if *he* is made later to realize the mischief, it is easy for him to accuse others.

NOTES UPON THE ESTIMATION OF CHLORINE IN ELECTROLYZED SOLUTIONS.

By L. M. NORTON, Ph.D.

The numerous experiments made during the last few years upon the production of chlorine, hypochlorites and chlorates by the electrolysis of solutions of chlorides, have brought into the hands of analysts solutions containing chlorine in several combinations.

It has been found that rapid volumetric methods for the determination of the combinations in which the chlorine existed were a necessity where such solutions were to be frequently tested, and the writer thinks that a description of the methods he has employed, while they contain little that is new, may be of interest to chemists who deal with similar problems.

By the direct electrolysis of aqueous solutions containing sodium chloride at ordinary temperatures, solutions are obtained which contain sodium chloride, sodium hypochlorite and sodium chlorate, and this general action is undoubtedly true in regard to other chlorides. A most careful examination has failed to show the presence of sodium chlorite in solutions thus prepared. If, however, the temperature is raised to 50°–55° C.[1] the solution contains mainly sodium chloride and sodium chlorate.

Such solutions may be very conveniently examined by the methods described below.

ESTIMATION OF CHLORINE COMBINED AS HYPOCHLORITE.

A quantity, 25 cc. for example, of the solution is measured out and titrated in the ordinary manner with a sodium arsenite solution of known strength. The solution after the titration must be preserved for the determination of the chloride present.

From the equation:

$$As_2O_3 + CaCl_2O_2 = As_2O_5 + CaCl_2$$

the quantity of chlorine combined as hypochlorite may be calculated.

[1] Rigaut, La Lumière électrique, **40**, 102.

ESTIMATION OF CHLORINE COMBINED AS CHLORIDE.

The portion of the original solution which has been titrated with sodium arsenite is now conveniently made up to a known volume and an aliquot part is taken. To this part a solution of pure potassium chromate is added, and the solution then titrated with silver nitrate of known strength. The presence of arsenic compounds does not interfere with this titration in any way; in fact, sodium arsenite may be used as an indicator in the place of potassium chromate, and equally good results may be thus obtained.

The figures furnished by the titration with silver solution give the chlorine present as chloride and hypochlorite together in the original liquid. By subtracting the chlorine combined as hypochlorite from the chlorine shown by the titration with silver solution, the chlorine present per cubic centimeter as chloride is readily obtained.

ESTIMATION OF THE TOTAL CHLORINE.

It might seem at first thought that the total chlorine could be obtained by the titration of the original solution which had been electrolyzed; but this is not the case, as during the electrolysis of a chloride solution there is a decrease in its volume caused by the rise in temperature due to the current, so that the contents per cubic centimeter of the solution before and after electrolyzing are not comparable, unless, in the latter case, the solution is carefully brought to its original volume by the addition of water. A further error is unavoidably introduced by a loss of chlorine due to its escape directly into the air during the electrolysis, a loss which the writer has always observed in the various forms of apparatus which he has used.

As the solutions under discussion contain chlorine in two oxidized forms as well as in the form of chloride, it is of course evident that for the determination of the total chlorine the simplest method will be the reduction of the oxidized chlorine compounds to the form of chloride, and a subsequent titration with silver solution. Various reducing agents have been recommended from time to time for the reduction of chlorates[1] to chlorides with a view to the subsequent titration of the chlorides with silver solution. After many experiments the writer has found that the most satisfactory agent for this reduction is a saturated aqueous solution of sulphurous acid. This was suggested by Rose,[1]

[1] Handbuch der analytischen Chemie, 2, 613, Leipzig, 1871.

and lately Engel[1] has called attention to its possible use in this connection. Chlorates are completely reduced to chlorides by boiling for a few moments with an excess of an aqueous solution of sulphurous acid, with or without the addition of nitric or sulphuric acid. Thus the following figures were obtained in the analysis of a sample of commercial chlorate of potash by reduction with sulphurous acid solution, after acidifying strongly with nitric acid, boiling off the excess of sulphurous acid, followed by titration with silver solution in the manner to be mentioned below:

Theory for pure $KClO_3$.		Found.
Chlorine	28.93%.	28.70%, 28.82%, 28.82%, 28.79%.

As during the reduction free acid is formed in the chlorate solution, even if it is not added at the beginning, the use of potassium chromate as an indicator, together with direct titration with silver nitrate solution, failed to give satisfactory results. The figures obtained, however, were entirely satisfactory if the reduced solution was treated with an excess of silver nitrate solution of known strength, ferric alum added as an indicator, and the excess of silver nitrate determined by titration with ammonium sulphocyanate solution in the manner recommended by Volhard.[2]

The total chlorine being thus obtained, the amount present as chlorate per cubic centimeter is easily found by subtracting from the total quantity the amount previously ascertained to be present in the forms of chloride and hypochlorite.

It is also practicable to determine the hypochlorite by means of an arsenite solution, the chlorine present as chloride and hypochlorite together by means of silver solution as given above, and the hypochlorite and chlorate by allowing them to act upon potassium iodide in the way ordinarily described in the handbooks; and from the data thus obtained the amount of chlorine present in each form can then be calculated. I consider this process less convenient than the one already described.

The writer has used still another method which gives good results, but it is less rapid than those mentioned. After the determination of the hypochlorite and chloride present has been made, the total oxidizing power of the original solution, due to the hypochlorite and chlorate

[1] Silva, Traité d'analyse chimique, 151, Paris, 1891.
[2] Annalen der Chemie, 190, 1.

contained in it, may be estimated by adding a known quantity to an excess of an acid ferrous solution, containing a known quantity of ferrous sulphate, boiling for a few moments in an atmosphere of carbonic acid, and titrating the unoxidized ferrous salt with potassium permanganate or potassium bichromate. The oxidizing power of the original solution can be calculated from the ferrous iron oxidized by it; and as the hypochlorite present is known, the chlorate present can be calculated.

ABSORPTION OF CHLORINE.

In testing the efficiency of electrolytic chlorine generators it is most convenient to absorb the chlorine produced in sodium hydrate solution. The amount of chlorine already present in the alkaline solution should be determined by a preliminary analysis. The alkaline solution which absorbs the chlorine will contain it mainly as hypochlorite, but also as chloride and chlorate. A known fraction of the absorbing liquid is taken, made up to 1,000 cc., and of this 50 cc. are measured and 20 cc. of sulphurous acid solution and 5 cc. of nitric acid added, the sulphurous acid removed by boiling, and the total chlorine is then easily found by the method described for the estimation of the total chlorine in the electrolyzed solution, and from this the chlorine originally in the solution must be subtracted.

I see no reason why the methods given above may not be applicable to the analysis of bleaching powder in cases where the estimation of total chlorine and chlorate is desirable.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
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MILK SUPPLY AND THE PUBLIC HEALTH.[1]

By WILLIAM T. SEDGWICK, Professor of Biology.

One of the noteworthy social changes of the present century has been the growth of cities and the relative increase of urban and suburban populations. These vary in density; but in all, or nearly all, cases they depart so widely from the more primitive rural or village form as to constitute an organization essentially altered and a condition in which the more primitive ways of life require to be much amended. This fact is quickly recognized in the social, legal, and moral relations; but on the physical, and especially on the sanitary, side it is often overlooked. The most fundamental sanitary effect of urban and suburban life is to make general and public the supplying of certain common wants which were previously special and private. Much that was essentially domestic, and under private supervision, becomes essentially foreign, and passes of necessity beyond domestic control. The water supply, once drawn from the domestic well or cistern, which was easily guarded, is now derived from a pipe filled, it may be, many miles away, from a source of which the individual householder has no knowledge whatever. The household wastes, once disposed of in the simplest fashion, now pass into common sewers through convenient openings in sinks and closets and sometimes sleeping-rooms. Even the supply of food becomes also essentially public. Animals are no longer slaughtered in the domestic barn, but in the public stockyard or abattoir. Flour, vegetables, fruits, and all the other necessaries or luxuries of life have passed, or are passing, beyond the pale of domestic supervision; and supervision by public experts must necessarily replace that of the individual or the family.

There can be no question that these changes have often brought with them marked sanitary improvements; but it is equally clear that

[1] An Address read before the Society of Arts, Massachusetts Institute of Technology, December 10, 1891.

the possibilities of extensive harm are in some cases distinctly enhanced. The introduction of a pure water supply can easily be shown to be followed by an improvement in the public health; but the contamination of such a supply may be followed by disaster far more universal than could possibly have happened upon the more primitive system of disconnected domestic wells. Other examples might be given, but this will suffice to show that public supplies are attended with public dangers.

The corollary is plain. The public, in surrendering the privilege of private control and supervision of their various supplies, should carefully provide and maintain for them the most expert public supervision. This is supposed to be done in the case of water supply and sewage disposal, though it is seldom done as it ought to be. The sources of the public water supply are generally well known and more or less guarded against pollution. The removal of sewage and garbage is usually fairly well attended to. Meat and other provisions receive some public supervision, though more might well be given. Decayed fruit is occasionally seized and destroyed. Even milk is examined, and dealers in watered milk are prosecuted. In most of these cases more ought to be done than is now done to protect the public, that is, to protect the private individual who, having surrendered the right of domestic supervision, is virtually at the mercy of dealers in public supplies. In particular his water supply, sewage disposal, gas supply, food supply, and especially his milk supply, demand the most careful public supervision. In this paper we shall consider the problem of milk supply and the public health.

At the top of the animal kingdom stand those animals which give milk. This property is so peculiar that it has been made the distinctive characteristic of the highest group of animals, which are named, accordingly, the *mammalia*, that is, those animals which have *mammæ*— the mammary glands or milk-producing organs. In the process of the domestication of animals, primitive man appears to have cultivated and developed this remarkable property, using the cow, the goat, the mare, the camel, the reindeer, and other animals, not only as beasts of burden for mechanical purposes, but also as direct bread-winners in the form of milk producers. In one stage of his development, primitive man lived with his animals, and moved about with them in their search for food. In this simple, migratory state, he occasionally found lands flowing with milk and honey, and, untroubled by the purity or freshness of his milk

supply, sang their praises for future generations. To him the only problem of milk supply was that of quantity; its quality was, as yet, unaffected by the barnyard, by the stable, by adulteration, by staleness, or by the centrifugal machine.

With his departure from the primitive and patriarchal state, man has come, at length, to dwell in dense masses called cities — places which do not flow with milk or honey, but, instead, with smoke and sewage. Under these changed conditions man has changed, and his food supplies have changed; but not so much in the nature of the raw materials, as in the treatment to which they are subjected before they are consumed.

Most of our foods today, fortunately, are purified by fire before they are admitted into our alimentary canals. Meat is purified by roasting; potatoes, by washing, peeling, and heating to a high temperature; cereals are powdered, sifted, moistened, and then purified by fire, in the oven or in boiling water; bananas, apples, oranges, pears, grapes, and nuts come to us hermetically sealed in dirt-tight skins or shells; cherries, berries, and such vegetables as celery and lettuce, on the other hand, we eat as we find them, raw — but only after a thorough cleansing.

Milk, however, to a great extent, man still uses much as his primitive ancestors used it; namely, in the raw or uncooked condition. And yet the milk of a modern city is altogether different from the milk consumed by primitive man. It is seldom fresh; it is often adulterated, and it is usually dirty. The stable, and travel more or less prolonged, have had their effect, so that the milk when delivered is often far along on its way towards decomposition. Here primitive man had the advantage, not only in his roaming, which made the stable unknown and the barnyard an impossibility; the cow, the goat, and the camel were his valued companions, and their milk was consumed while it was still fresh. For him it was not transported hundreds of miles by railway, or deposited at the door of his tent at an hour of the morning suggestive of freshness, but really long after it had parted from the cow.

It is instructive to consider somewhat carefully the source of the milk supplied to a modern city, and its history, from the time it originates in the udder of the cow until it is consumed by the city infant or adult. Milk is an animal secretion prepared in much the same way as are the tears, the bile, the gastric juice, the urine, the sweat, and the

saliva. It is secreted in the mammary gland, or udder, by certain epithelial cells which manufacture it from the blood. During the process of secretion these cells break open and contribute of their own substance to the mixture of water, salts, sugar, and fat, which constitute milk. At every milking a portion of the udder substance, the actual animal stuff, is given off; and thus it comes to pass that milk is strictly an animal food, like an infusion of muscle or of liver. It is unnecessary to say more to make it plain that it is highly unstable, highly decomposable, highly putrescible.

As every one knows, milk is drawn from the cow by a peculiar operation called "milking." As it is ordinarily conducted, this act requires the operator to be seated before the udder for some moments, probably never fewer than five. A pail is held between the knees of the milker, the teats are seized by the hands, and by a peculiar peristaltic action the milk is squeezed out from the teat, which is instantly filled again from the secreting gland. The operation requires much shaking of the udder, and very vigorous movements of the milker's arms. Only those who know can realize the unclean condition of many milkers, many stables, and most cows. I need only remark that a wide-mouthed pail held under the shaken udder necessarily receives whatever is dislodged above it. The sediment in milk pails and milk cans has been examined microscopically and found to consist of hairs, dandruff, flakes of dried excrement, with less objectionable matters, such as hay, in large abundance.

The milk thus drawn, and already more or less charged with what may be euphemistically called "dirt," is next strained through a coarse filter of some sort, by which the larger particles are removed. It is then cooled to check the process of decomposition and thus increase its keeping qualities; and eventually it is shipped to the city, either by wagon or by railway. More or less of time here necessarily intervenes before the milk is finally placed in the proper cans and delivered early in the day to the consumer. In Boston the milk is usually from thirty-six to forty-eight hours old before it is thus delivered. In the consumer's hands several hours more often go by before the milk is actually used. In the case of people too poor to have ice, resort is usually had to groceries which keep one or more large cans of milk on ice. In some parts of Boston milk is doled out from such groceries by the cent's worth.

The amounts of milk supplied to cities are very large. In Lowell,

for example, about 30,000 quarts are received and sold daily. Lowell has 80,000 inhabitants, so that for every man, woman, and child, Lowell receives three eighths of a quart daily. Much of this is used, of course, for cooking; but much is also fed in the raw, uncooked condition, to infants, children, and invalids. The amount received in Boston is as much as 50,000 gallons per day. We may reasonably ask what is the origin and sanitary condition of this immense and important public supply.

A considerable portion of the Boston milk supply is produced upon farms adjacent to the city; a very small portion within the city itself; but the greater part originates upon distant farms in Massachusetts and New Hampshire, from which it is brought into the city by railroad. On its arrival it is sold to small dealers or peddlers, by whom it is kept for a longer or shorter time, and ultimately delivered to the consumers. The consumers, of course, have no knowledge whatever of its origin or previous history when it is deposited at their doors. They do not even know, in most cases, whether it has come from their own State or not. Obviously the consumer can exercise at present no control over the cows or the stable which produce his milk supply. He may, however, investigate its condition on its arrival.

It is well known that milk, especially as it is found in large cities, is usually swarming with living bacteria[1]. Most of these are probably harmless vegetables; nevertheless they constitute a host of organisms entirely foreign to the milk itself. The so-called "pure" milk ordinarily obtained in Boston, even when it has not been tampered with, frequently contains in a teaspoonful millions of these microscopic vegetables luxuriantly thriving in a soil most favorable for their development. Moreover, although themselves invisible, they do not fail to produce obvious effects. The souring and chemical decomposition of the milk which soon appear are due exclusively to their vital activity, and the milk which was originally sweet, and which in their absence remains forever sweet, becomes, owing to their activity, sour and chemically decomposed. In this condition it has important uses, but it is no longer "fresh" or "sweet" or "normal" milk. Under the worst conditions milk may actually be delivered to the consumer nearly or quite sour; and we have ascertained that in Boston it is

[1] The statements that follow are drawn chiefly from "A Bacteriological Examination of the Boston Milk Supply," by W. T. Sedgwick and John L. Batchelder, Jr. Boston Medical and Surgical Journal, January, 1892.

regularly and systematically delivered to the great majority of consumers comparatively well advanced towards this condition.

It is with this aspect of the subject only that we shall deal in the present paper. We shall show, in the first place, that the normal milk of the cow contains no ordinary putrefactive bacteria; we shall next seek to discover the source of the swarms of bacteria which infest ordinary city milk; we shall then briefly consider the bearing of these phenomena upon the public health, and the possibility of remedying the present conditions.

It will be observed that we are not now dealing with the specific germs of disease. It is well known that milk is an excellent food for many kinds of bacteria, and this, added to the fact that it is one of the cheapest and most trusted of food-stuffs, makes it, when infected with pathogenic organisms, a peculiarly dangerous vehicle for the transmission of infectious diseases. Cow's milk, when once infected with the specific organisms of disease, is probably a medium far more dangerous than an equal volume of infected water, since, for many bacteria, it is a much more favorable soil. With this aspect of the subject, however, we shall for the present have little to do. We desire first simply to show how far the milk in Boston regularly differs in bacteria from the normal, and to discover the causes of this difference. The methods employed for this more general inquiry have not been adapted or intended for the discovery of the exceptional cases of contamination of the milk with the specific germs of such diseases as tuberculosis or typhoid fever.

Chemical examinations, both official and unofficial, of the milk supply of Boston, have been, and continue to be, repeatedly made; but no systematic and extensive bacteriological examination has hitherto been reported. It appeared to us therefore highly important, at least from the scientific standpoint, to inquire what information might be gained by a series of bacteriological examinations systematically conducted. To this end, during the past two years we have made many hundreds of examinations of Boston milk, of which only the principal results are here published.[1]

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[1] An abstract of the original paper was read by J. L. Batchelder, Jr., before the Boston Society of Medical Sciences, March 31, 1891. The data upon which it was based were largely obtained as results of an investigation conducted under my direction by Mr. Batchelder, and presented by him, in 1890, as a thesis for the degree of Bachelor of Science in the course in Biology at the Massachusetts Institute of Technology.

In order to discover the bacteriological changes which milk undergoes in its journey from the cow to the consumer we have made a series of examinations of milk as it flows from the cow; of milk as it is carried from the stable; of milk as it is found upon the tables of well-to-do country families; of milk as it arrives in Boston; of milk as it is sold from wagons, in groceries, etc.; and finally, of milk as it exists upon the tables of well-to-do city families. The main object throughout was to discover the source and significance of the bacterial swarms which infest city milk. To this end our experiments began with the cow.

I. NORMAL (UDDER) MILK.

That milk must be regarded as *normal* which is drawn from the teat of a healthy cow. In such normal, or udder milk, we have found no trace whatever of bacteria. The same result has been obtained by some previous observers, while others have found it difficult or impossible to obtain milk from the cow free from all germs. All observers, however, agree that very few, if any, organisms are present in normal cow's milk, and that the germs sometimes observed have probably been due to defective manipulation. Without special precautions, we also have found it extremely difficult to draw milk from the cow's udder *entirely* germ-free, and at first we failed to get it absolutely sterile. By a special method, however, we have succeeded in drawing, with ease, from the cow's udder, absolutely sterile milk. From this fact and the similar observations of others, there need be no question that the normal milk of the cow is free from bacteria. A consideration of the mechanism and physiology of secretion will show that this, for the healthy cow, is entirely reasonable. Normal cows secrete milk; they do not secrete bacteria.

A method by which this fact can be easily established deserves description. If one seeks to draw normal milk, germ-free, by hand, and milks in the ordinary way into a sterilized bottle, he will usually fail to obtain sterile milk. The reason appears to be that the act of milking shakes the udder and throws down from the skin of the animal a shower of dust and germs which pass into the bottle with the milk. By the use of the silver catheter or so-called "milking-tube" of veterinarians, however, this disturbance is completely avoided. The milk flows, without action of any kind on the part of the observer, in a strong and steady stream, of which a portion may be easily caught by a merely instantaneous exposure to the air.

The precise method of procedure is as follows: A clean cow is selected, and the operation should be attempted only in a clean stable. Care being taken to avoid disturbance of the bedding (if any), the skin of the hind quarter and that of the udder are washed. The teats are then wiped dry, and milking in the ordinary fashion is begun in order to start the secretion. A pause is then made, and while the dust of the stable settles, the milk accumulates in the udder. The catheter, previously sterilized by heat, is drawn from a plugged test tube and cautiously passed through one of the teats into the udder. The milk instantly flows away from the resting udder, through the catheter, in a strong and continuous stream, from which a series of samples can readily be taken. Many of these, in our experiments, proved, if planted at once, perfectly sterile. Others showed a very few colonies, which were plainly due to aërial contamination.

We have not sought to determine the presence or absence of tubercle bacilli in normal milk, as this was foreign to our investigation. At the same time it is clear that this method might well be applied in such an investigation. The detection of tubercle bacilli in milk has hitherto depended, not on cultures, but upon the differential staining of cover-glass preparations, and upon the infection of guinea pigs with cow's milk. These experiments have sufficed to show the presence of such bacilli in the milk, but they do not tell us whether the bacilli have come from the udder or not. The mere detection of the germs of tuberculosis in cow's milk does not prove that these have proceeded from the cow's udder. They may have come originally from the saliva or excrement of the cow, and have fallen in during the act of milking. By the method just described, however, it might perhaps be found possible to obtain more valuable results, and even to make test cultures of diseased milk.

II. PURE COUNTRY MILK.

In the previous section we have shown that healthy cows give milk which, examined by the usual methods, is absolutely free from germs. It appears, therefore, that the normal milk of the cow is milk not only undecomposed, but free from the germs of decomposition. "Pure" milk may possibly be defined as milk which has not been watered or adulterated. It is sometimes so defined. But normal milk cannot be defined in this way, and in the considerations which will follow the high standard of normal milk must be kept in view. Clearly, also, the

cow is not the source of the hosts of bacteria regularly observed in Boston milk. We therefore turned to the examination of milk as it is found upon the tables of well-to-do country families; that is, to the study of unquestionably "pure" country milk. Such milk is generally comparatively fresh, and is often served only a few hours after the milking. Nevertheless, our results show that it is generally rich in bacteria, though the numbers are much smaller than in ordinary city milk. We then made the following experiment: milk was drawn from a clean and well-kept Holstein cow in an unusually decent stable. The normal milk of this cow had already been repeatedly proved to be sterile. Milk drawn by hand from this cow with great care into sterilized bottles, and planted quickly, yielded, as an average of several trials, 530 bacteria per cubic centimeter. When, however, the milkman used the ordinary milk-pail of flaring form, seated himself with more or less disturbance of the bedding, and vigorously shook the udder over the pail during the usual process of milking, we found that the numbers were very much higher, namely, an average of 30,500 per cubic centimeter, at the end of the milking. When such milk is found upon the tables of country families a few hours later, it naturally shows still more bacteria, doubtless because those with which it was seeded have had time to multiply. The average of fifteen such samples from the tables of families in Jamaica Plain, Cambridge, and Auburndale, was 69,143 per cubic centimeter. In these cases, moreover, the conditions of the cows and of the stables were exceptionally good, while the milkmen were much more than ordinarily clean and careful.

It follows from these results that there are two principal sources of the bacteria in milk; namely, contamination during the act of milking, and the natural multiplication of the bacteria thus introduced during the interval between milking and the consumption of the milk. The result of these investigations was to show that even under the most favorable conditions cow's milk as ordinarily drawn becomes, almost necessarily, infested with hosts of putrefactive bacteria at the very outset. Under worse conditions, with unclean stables and dirty milkmen, to say nothing of half-cleaned pails and cans, it is easy to understand why milk swarms with bacteria; and, if we allow time also, the wonder is, not that it contains so many germs, but rather that it is still potable at all.

When we reflect upon the indescribably filthy condition of many cow stables; upon the fact that the cow's udders and flanks are not

infrequently covered with flaking excrement; upon the quality of the men employed to do the milking, etc., etc., it becomes a simple matter to understand how this rich animal fluid — sterile at the start, but drawn by unclean hands into half-cleaned pails, and meantime sprinkled from above by the dust of the stable, by hairs, dandruff, dirt, and particles of excrement from the skin and udder of the cow vigorously shaken by the milker or brushed by his hat — becomes infested with organisms. That these multiply swiftly and enormously in the warm and rich fluid, well aërated by the act of milking, is also a natural consequence of favorable conditions.

III. BOSTON MILK.

If, now, we turn to the condition of city milk as exemplified by that delivered in Boston (and we have no reason to suppose that Boston is worse in this respect than other large cities), we find, as might be anticipated, a marked increase in the numbers of bacteria over those found in country milk. The average number of bacteria found in fifty-seven samples of Boston milk in the spring of 1890 was 2,355,500 per cubic centimeter (roughly equal to a cubic one third inch or to one small thimbleful). The samples came from different parts of the city. Thus:

Charlestown,	8	samples	averaged	4,222,500 per cc.
Jamaica Plain,	10	"	"	3,259,600 "
Roxbury,	17	"	"	1,874,300 "
South Boston,	9	"	"	2,778,000 "
North End,	6	"	"	708,100 "
Back Bay,	7	"	"	1,189,800 "

The samples were obtained directly from the milk wagons and were planted at once. We are indebted to Dr. Charles Harrington, Milk Inspector of Boston, for much courteous assistance in procuring these wagon samples.

From groceries we obtained sixteen more samples, in which the average number of bacteria was 4,577,000 per cubic centimeter. The groceries usually have older milk than that found in the wagons, which readily accounts for the higher numbers of bacteria.

Ten samples collected from well-to-do families upon the Back Bay showed an average of 1,438,000 per cubic centimeter. The lowest number found in Boston milk supplied in the ordinary way was 30,600 per cubic centimeter. With special care, however, milk can be, and

now is in a few cases, regularly delivered in Boston with much greater freshness and purity than these figures indicate.

It will be seen at a glance that the milk supply of Boston is very rich in bacteria. Much of the milk arrives from very distant points by rail, and is therefore necessarily more or less stale. Forty-four samples of this so-called "railroad" milk from one dealer showed that the milk, even upon its arrival, contains on the average more than 500,000 bacteria per cubic centimeter. The extremes in this case were 5,664,000 and 2,200. In another set of ten samples the milk, on its arrival in Boston, averaged 371,000 per cubic centimeter.

Our experiments have been repeated in 1891, and the results prove that the figures for 1890 are not exceptional. There are some indications that, as might perhaps be expected, watered milk contains fewer bacteria than unadulterated milk. "Skimmed" milk, on the other hand, probably on account of its staleness, is sometimes very rich in organisms, though this is not always the case.

IV. INTERPRETATION OF THE RESULTS.

We have now shown that the normal milk of the cow is free from bacteria. We have also found that the milk supply of Boston is exceedingly rich in bacteria. We have further discovered that these are principally introduced during the operation of milking in unclean stables, and that they afterwards multiply enormously in the milk, in which they effect important changes of decomposition. Two principal conditions thus coöperate to cause the extraordinary abundance of bacteria in Boston milk; namely, *uncleanness* and *staleness*. The former furnishes the seeds of decomposition; the latter the time for their development. Their coöperation produces a fluid widely different from normal cow's milk.

It remains to inquire what is the probable effect of this condition of the milk supply upon the health of the community. Here we are very much in the dark. There can be no question that most of the milk is consumed when cooked, and even when raw, without the least apparent injury and with great apparent benefit. But it is probably also true that the use of stale and partially-decomposed milk, charged with living bacteria, has its effects upon invalids and children, and particularly upon infants, and that these effects are not always beneficial. It is possible that one explanation of the great mortality of children under five years of age, and especially of bottle-fed children, is to be sought for in this direction.

It will not do to argue, because healthy adults drink polluted and stale milk without obvious injury and with evident benefit, that invalids and infants may safely do the same. In order to learn the consequences of a battle, the investigator must examine not merely the survivors; he must consider also the fallen. If it be admitted that city infants, children, and invalids require normal cow's milk, it cannot be denied that they are now rarely, if ever, fortunate enough to get it. Many parents who are fastidious to the last degree concerning their own wine or table linen, provide for their children cow's milk which is both stale and filthy. It is safe to say that if our soups or our drinking water were drawn from cows, in remote and obscure stables, by ordinary milkmen, and shipped, manipulated, and delivered as our milk supply is, we should appreciate and resent the pollution. At present, however, so far as mere pollution is concerned, it is probably true that milk is actually improved by the addition of pure water. The public inspection of milk in America is usually directed mainly to the prevention of fraud; rarely, if ever, to the question of pollution, or, except in a very general way, to the protection of the public health.

"One point deserves in the future much greater attention. This is the *pollution* of milk. No food material can be so much polluted as milk. If any one will compute how much cow's excrement an infant swallows, and how much excrement an adult consumes in drinking the sewage-polluted water of the Isar, he will find that the latter is by far the better off."

Dr. Sohlet, the author of the foregoing paragraph, has lately urged that milk be examined not only in respect to its solids and fats, but also as to its contents in filth, after a method employed by Professor Renk, of Halle. The latter found that the public milk supply of Halle was polluted by very considerable amounts of filth, which settled to the bottom of the vessel containing the milk, and upon microscopical examination proved to be largely cow's excrement. The average of thirty tests showed fifteen milligrams per liter, or fifteen parts per million, of such filth deposited by the milk of Halle. In Leipzig milk, Renk obtained 3.8; in Berlin, 10.3; in Munich, 9.0 milligrams of similar filth deposited per liter. He found the bacteria in the Halle milk supply varying from 6,000,000 to 30,000,000 per cubic centimeter, a result agreeing well with its unclean condition. It may be remarked in passing that the sewage of American cities seldom contains, on the average, more than 1,000,000 bacteria per cubic centimeter.

V. REMEDIAL MEASURES.

It is clear that the improvement of the conditions which now exist must consist in greater cleanliness in milk production and less delay in its delivery. So far as pollution is concerned, it is possible to make great improvements by requiring cleaner stables; those which exist at present being often (from the standpoint of mere decency) simply abominable. The cows might also be kept much cleaner, and, if necessary, washed above and upon the udder with warm water before milking; or, at least, cleaned and groomed as horses have long been. The milkers might be compelled to have clean hands, and taught how to avoid pollution of the milk. That the milk-pail might be improved, and even kept partly covered, has been demonstrated by at least one invention now actually in use.

If the milk could thus be kept decently clean and nearly free from bacteria — the seeds of decomposition — and then quickly chilled to a low temperature, its decay would be very slow, and its "keeping qualities" might be greatly increased. It could afterwards be transported, over even long distances, without damage, provided only that a very low temperature were constantly maintained. Under existing methods, however, quick delivery is essential, since the milk is richly seeded with filth at the very outset, and only imperfectly chilled during its journey to the consumer. To permit of all these improvements it is possible that a higher price might have to be paid for clean and fresh milk, as is now done for fresh-laid eggs.

Thus far but little has been said of the dangerous possibilities of milk as a vehicle for the specific germs of disease. Stress has here been laid upon its staleness and uncleanness, and there can be no question that the dweller in Northern cities suffers from a milk supply affected with these conditions. The visitor to some Southern or tropical cities may witness a very different state of things. In Naples, for example, cows are driven through the streets, and the buyer of milk may have the satisfaction of seeing it drawn at his very door from a clean cow, to which the stable is unknown, while he himself witnesses the operation. The curious anomaly actually appears to exist that the milk supply of Naples is fresher, purer, and cleaner than that of Boston or New York. In Naples, in spite of urban life, the primitive method of milk supply still prevails.

But we are exposed not only to unclean, but also to infected milk, that is, to milk not merely polluted with filth, but also infected with

the germs of specific diseases, such as tuberculosis and typhoid fever. Numerous epidemics of the latter have been traced to infected milk, and it is now believed that the former is often conveyed in this most favorable medium. Whether scarlet fever and diphtheria are often carried in milk or not is still a question under discussion, but there is reason to believe that they may be so carried.

In view of all these facts we may well ask ourselves whether in the place of private care and watchfulness we have provided any efficient or adequate inspection of the stables from which the milk supply comes, or any even approximately adequate supervision of it after its arrival. There is at present, so far as I am aware, no systematic inspection whatsoever of the farms, stables, or herds from which public milk supplies come. After milk has arrived in the city, inspectors protect us more or less effectively from its adulteration with water. The dwellers in cities take such milk as comes, asking no questions. They have surrendered their primitive safeguards, and have placed none in their stead. By the present system of inspection we are protected from fraud, but not from filth or disease. Public milk supplies may not legally be watered, but they may be stale, or polluted, or infected.

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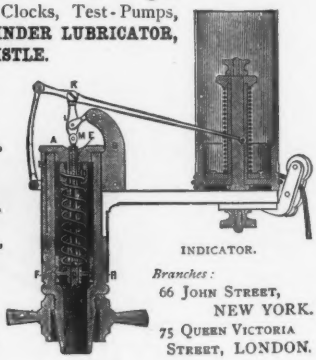

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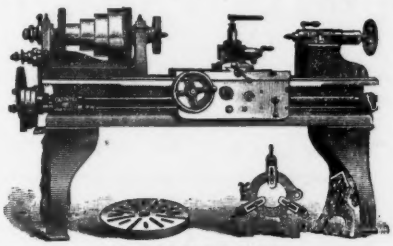

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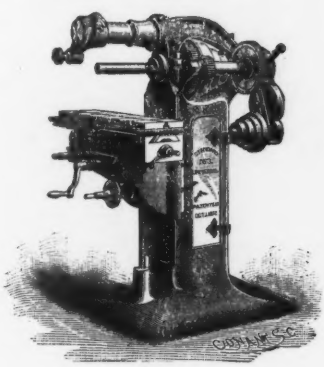

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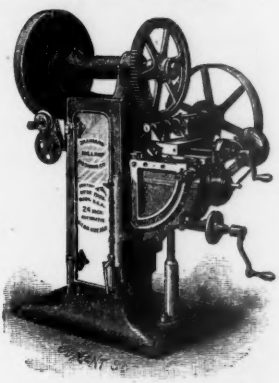

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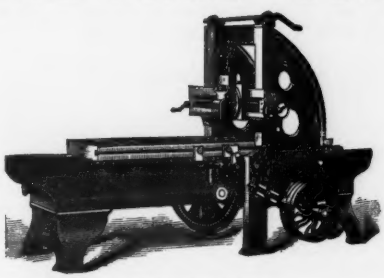

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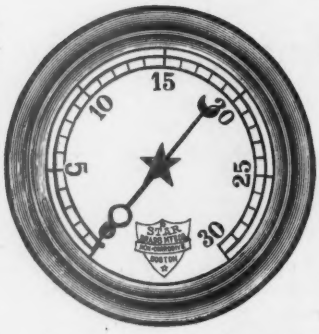

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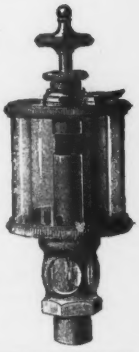

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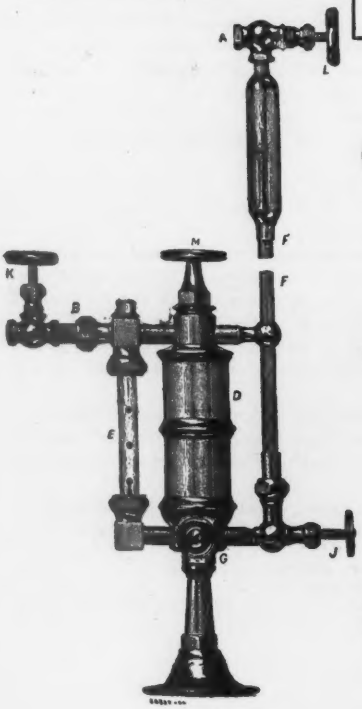

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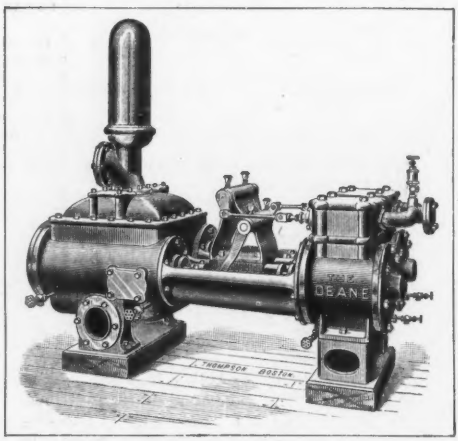

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